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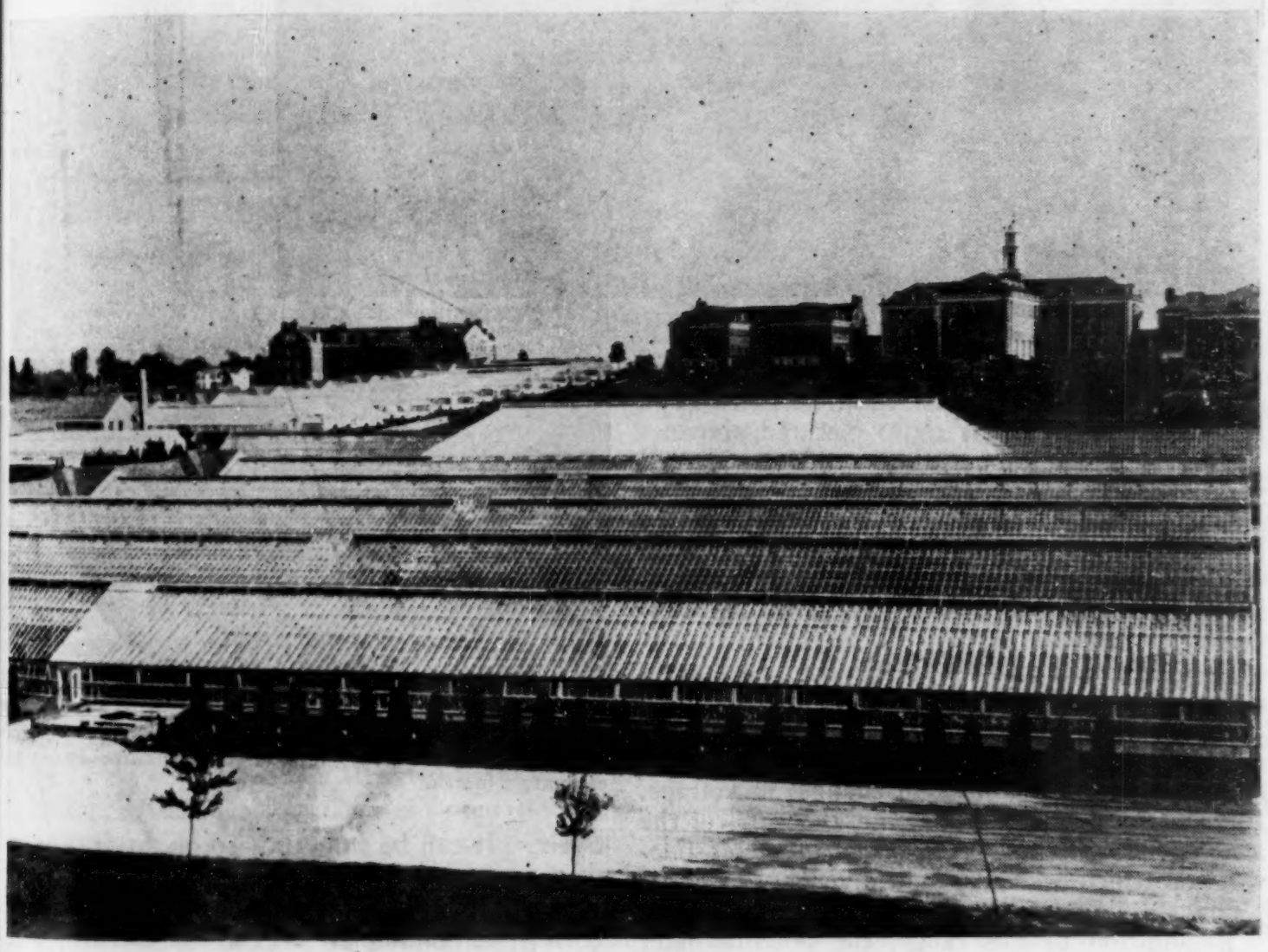

Plant Industry Station, Agricultural Research Administration, U. S. Department of Agriculture, Beltsville, Maryland, including a section of the extensive greenhouses.

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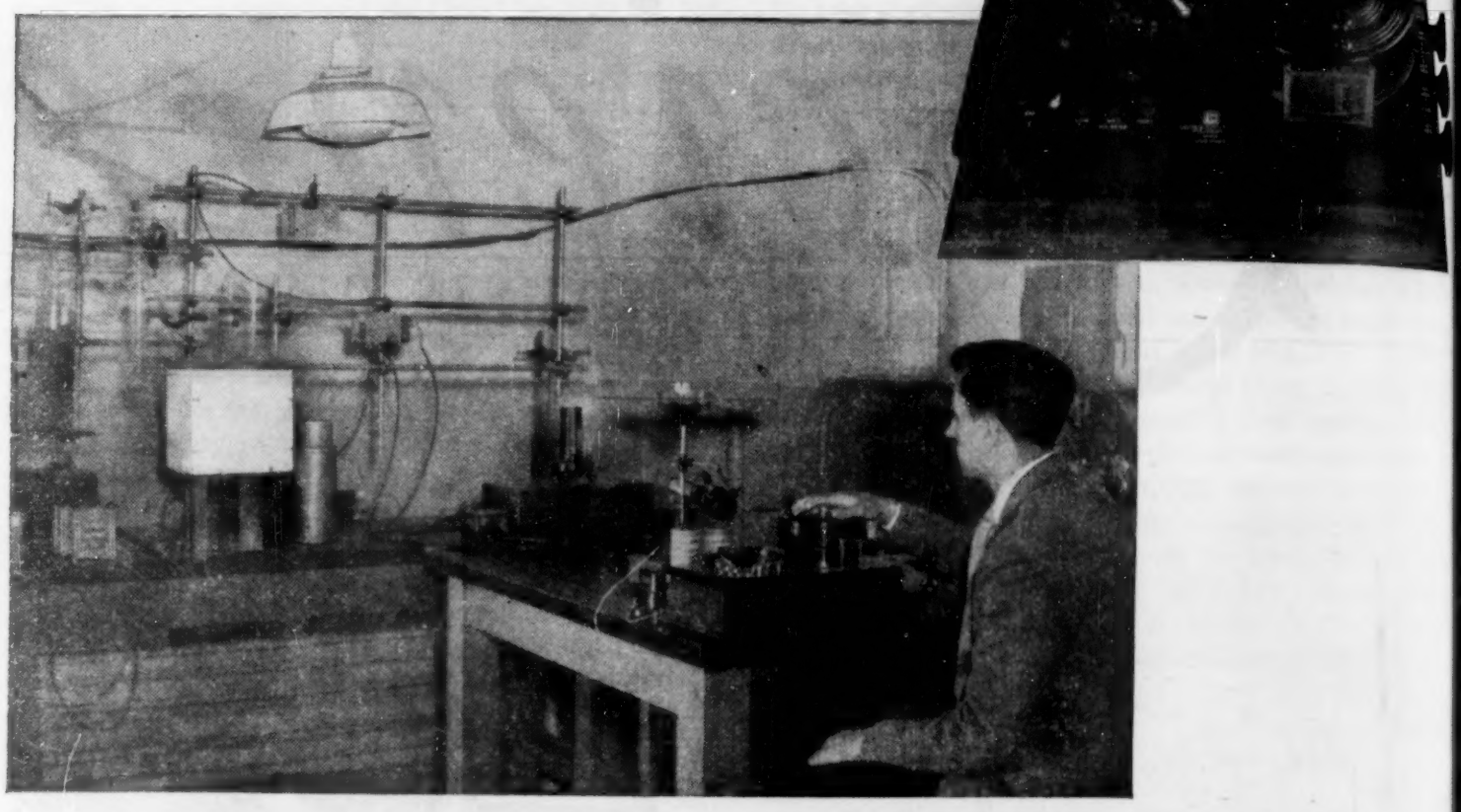
Discharge Through Gases

Leonard B. Loeb

Low Pressures Measured Electrically Using Type K Potentiometer



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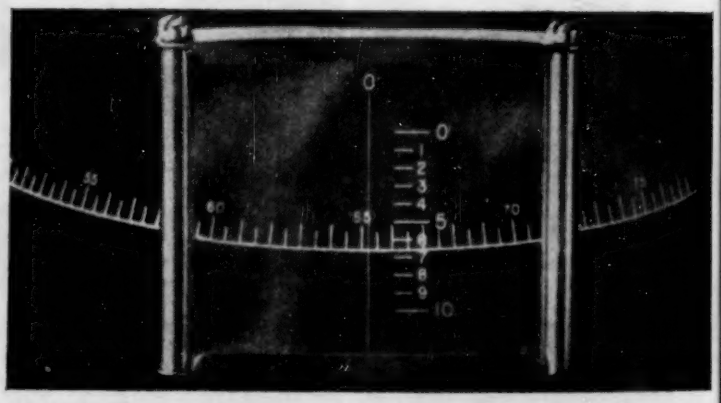

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Discharge Through Gases

Leonard B. Loeb

Department of Physics, University of California, Berkeley

IT IS PERHAPS TIMELY THAT "DISCHARGE Through Gases" should be presented before the Institution, since 1947 marks closely the 50th anniversary of the proper scientific beginning of this field of study. It is particularly appropriate that it be presented before the Institution of Electrical Engineers, since it is from the research laboratories of England that most of the early significant advances have come. The subject has a peculiar and important interest for the electrical engineer, in that we live and operate in a gaseous atmosphere. Thus, it is not surprising that limitations set by the electrical properties of the atmosphere and, in particular, its electrical breakdown are in this electrical age of considerable practical importance. When it is recognized, furthermore, that the peculiar properties of electrically conducting gases furnish important elements in modern technical advance in the form of illuminating, rectifying, and welding agencies, and agencies for electrical counting, electrical switching, and the amplifying of electrical currents, the importance of this field to the engineer is adequately established.

With the discovery of X-rays late in 1895 there came to hand a medium of producing sufficient conductivity in gases that proper study could be initiated, and in the next 10 years significant advances were made by that brilliant international group of young physicists working under Sir J. J. Thomson at the Cavendish Laboratory. As time went on, these investigators moved elsewhere and carried further the researches begun at Cambridge.

In these first 10 years most of the fundamental properties of gaseous conduction were delineated, and a general understanding of the subject was achieved. At the end of this period the lure of the exciting developments in the fields of atomic structure attracted the attention of many of these brilliant workers and drew them into other channels of endeavor. The loss of active workers, however, does not entirely account for the period of relatively slow development of the subject which followed; for it must be clear that the initial studies had gone as far as the existing knowledge and techniques of the period permitted. The later development of the Bohr atom, the discovery of the elastic electron impacts, excitation and ionization functions, inelastic impacts of the second class, and various mechanisms of secondary electron emission were essential to proper interpretation. Likewise, the vast improvements in physical techniques such as high vacua and outgassing, achievement of gaseous purity, measurement, the achievement of electrical pulses of short time intervals, and the development of high-frequency oscillographs, were perhaps even more essential to further advance.

It is accordingly not unexpected to discover that the mechanisms derived from earlier study, while correct in outline, were in many cases seriously wrong in detail and that in general the simple pictures which have now become fixed in the textbooks need radical revision. It is thus my purpose to sketch the significant advances in the various aspects of the subject and—perhaps more important—to indicate the tasks which remain to be done.

In the early studies much interest was focused on the nature and mobility of the positive and negative ions in gases, with particular emphasis on the question of whether the ion was molecular or consisted of a large cluster of molecules. Inadequate techniques and lack of gaseous purity led to completely confusing and contradictory results. When it was realized that ions could change charge, had specific affinities for certain atomic or molecular species, and that in one second of time an ion could collide with some hundred millions of molecules, the confusing results became comprehensible. Hence, in order to study ions of a known constitution with the controlled gaseous purity obtainable, it was required that ions that did not change charge be observed in short periods of time. Beginning simultaneously in the laboratories of Loeb (*45*) in Berkeley and Tyndall (*88*) in Bristol, the recognition of these difficulties led to a satisfactory solution of the problem. Particular credit must go to Tyndall and his school of able investigators who, over many years of tedious study, developed the techniques which ultimately clarified the problem (*86*). This work together with the recognition that dielectric polarization and electrostatic attraction play a role in giving a fictitious collision cross-section, independently of the size of the ion cluster, has now satisfactorily brought the theory of ionic mobilities into agreement with experiment (*48*). Notable among these theories is the one published in 1905 by Langevin (*41*) and not recognized until rediscovered by Hassé (*25*) in Bristol. Clustering of a limited and often very specific sort does occur, but is not of the extensive character originally

The 38th Kelvin Lecture was delivered in London on April 24, 1947. Through the courtesy of the Institution of Electrical Engineers *Science* has been granted the privilege of printing this lecture, which appeared in the August issue of Part I of the Journal of the Institution.

assumed (*87*). While much in detail yet remains to be done, the resulting information will be of value primarily to the physicist and physical chemist interested in atomic and ionic force fields and structure. It will be of no great importance in engineering applications. What the engineer must, however, recognize is that gaseous ion mobilities with ions of more than 10^{-4} second of age, except in a few special cases, can have significance unique to the particular gas conditions of a given measurement only. Thus, most values given in tables are not of general applicability even in the supposedly same gases, but may be used in roughly predicting ionic velocities in order of magnitude in practical application.

The coefficients of diffusion of ions are related to their mobilities by the classical expression, $k/D = Ne/P$, where k is the mobility; D, the coefficient of diffusion; N, the number of molecules per unit volume; e, the charge; and P, the pressure. Thus, what has been said about mobilities likewise can be said of the coefficients of diffusion with little modification.

One of the most important problems in the study of gaseous conduction and breakdown hinges on the values of the velocities of electrons in electrical fields in gases and, more in particular, on their energy distribution and their average energies. As early as 1910 it began to be recognized by Kovarick (*38*) and Franck (*21*) that in certain gases electrons generated by an ionizing agency remained free, and that even in air at lower pressures the electrons did not at once attach to molecules to yield negative ions. By 1913 Townsend (*84*) and his pupils had devised a means for studying the mobilities of electrons in gases and evaluating some sort of an average value of their energies, using the ratio of mobility to diffusion noted above. Today it is the only existing method of even roughly evaluating the energies of the electrons and relating them to the experimental parameter, X/p, the ratio of field strength to pressure, which is the determinative quantity in most measurements of gaseous behavior. In 1920 Loeb (*44*) began investigations on the conditions both of attachment of electrons to molecules in gases and of their velocities, extending the velocity measurements to higher pressures and lower X/p values, and on somewhat purer gases than Townsend. This work has been continued over the years by Loeb's students, notably Wahlin (*90*), Cravath (*12*), and later by Nielsen (*61*) and Bradbury (*8*). Today it can be said that the problem of negative ion formation is fairly satisfactorily solved, as will later be indicated, while satisfactory experimental data on electron velocities over a wide range of pressures in some standard gases now exist. This is due largely to Bradbury and Nielsen's work, using Loeb's electron filter, redesigned by them as an electrical shutter. With techniques available since World War II the velocity measurements could easily be extended in range and accuracy if required.

Stimulated by the work in the early 1920's, together with the new observations concerning the mean free paths of electrons in gases determined by Ramsauer (*66*) and later by Brode (*9*), the way was open for a theoretical study of electron velocities. This was initiated by Compton (*11*), who extended his earlier approach with classical kinetic theory procedures to give a fairly satisfactory theory of velocities of electrons of energies of below the threshold for excitation and ionization. In thus proceeding, it had been assumed that electrons moving in a field in a gas had the Maxwellian energy distribution characteristic of thermal equilibrium. Following a study of Hertz (*26*), Druyvesteyn (*17*), of the remarkable team of physicists active at the research laboratories of the Phillips Company in Holland, derived the energy distribution for electrons in gases below excitation energies with *constant* electron mean free paths. He showed that the energy distributions did not exactly follow the Maxwell law, although the law which he derived was not very radically different. This was independently rederived, using more general procedures in kinetic theory, by Cravath (*13*), Davydov (*15*), and Morse, Allis, and Lamar (*59*). Experimentally, however, the point at which Bradbury and Nielsen's values of the velocities began to deviate from the theoretical values given by Compton for different gases indicated to these workers that the departures of the energy distribution from the Maxwellian, or even Druyvesteyn form, was much more radical than anticipated. They ascribed this to the fact that the Ramsauer electron free paths for some gases increased or decreased markedly with electron energy in the regions studied and roughly indicated how it operated. The rigorous calculation up to ionizing energies was carried out by Allis and Allen (*1*) to include variable Ramsauer free paths, using a method initiated by Pidduck (*63*) and further developed by Morse, Allis, and Lamar.

The fact that the departures from the conventional distribution law are so radical and that the distribution varies extensively over the energy ranges experimentally covered makes it impossible either to interpret the meaning of the Townsend evaluation of average electron energy in terms of X/p or to apply these to an analysis of the ionization and excitation functions. Thus, probably the most important fundamental problem outstanding today is in the evaluation of electron energy distribution functions in various gases over extensive energy ranges and the relation of these to the experimental parameter, X/p. At present I see no method of approach to the solution of this problem except through the laborious and intricate method of calculation initiated by Smit in Holland (*74*). Smit has shown how to derive these functions and has calculated them fairly rigorously for helium for only four values of X/p. This work must be extended unless experimental techniques can be devised.

The increase in our knowledge of atomic structure, together with improved techniques, led to the solution

of the problem of the formation of negative ions from free electrons and molecules. The work initiated by Loeb and Wahlin in 1920 and carried further by Loeb in 1923 indicated the essential soundness of the theory for this process, suggested by Thomson in 1916 (77). This work led to an estimate of the values of the probability of attachment of an electron to a molecule to make an ion for some gases. In 1925 V. A. Bailey (*3*), then at Oxford, showed experimentally that the probability of attachment in air decreased as the electron energy in the gas increased—a condition not recognized by Thomson. With the invention of the electron filter by Loeb, Cravath and later, more successfully, Bradbury (*7*) were able to measure the character of the process of the formation of negative ions. This was found to be a diversified and, in some cases, a complicated process. In only two molecular gases, O_2 and SO_2, do electrons directly attach. In these the process is one in which the electron attaches to the molecule, the low heat of ion formation going to vibrationa' energy of the molecular system. Unless in a relatively short time the molecule can lose this energy in impacts with other molecules, the electron will reabsorb its energy and leave the ion. The problem has been successfully attacked wave mechanically by Bloch and Bradbury (*5*), and the theory agrees with Loeb's findings (*46*) as to the energy of attachment. In other gases the energy of attachment to one of the atoms of the molecule, together with the energy of the electrons, suffices to dissociate the molecule and form the ion in a three-body process, the residual energy being taken up as kinetic energy of separation. While radiative capture of an electron yielding negative ion formation is possible, the probability is so small that ion formation takes place, if at all, only by three-body processes where the excess energy is removed in the kinetic form. As shown by Massey (*55*), certain spectroscopic states of atoms and molecules have negative heats of ion formation, and in these no electron attachment occurs. Notable among them are the $'S_0$ and $'\Sigma$ states (*7*). In such gases electrons remain permanently free. While much work still remains to be done, the data to be gained are of value primarily to those interested in atomic and molecular structure, and the present information is adequate for the engineer.

While the process of recombination of negative ions or electrons with positive ions, to annihilate the charges in the gas, was one of the first subjects studied in 1896, it has proved to be one of the most difficult and stubborn of all the problems to interpret. The basic equation for recombination reads that the loss of ions by recombinations dn in a time dt out of n ions of each sign per $cm.^3$ is $dn = -\alpha n^2 dt$. Its solution indicates that, starting with n_0 positive and negative ions per $cm.^3$ at $t = 0$, there will be $n = \frac{n_0}{1 + n_0 \alpha t}$ ions remaining after a time t. The quantity α is called the coefficient of recombination of ions. Early work by Thomson and Rutherford (*79*), Townsend (*80*), and especially Langevin (*40*) derived the order of magnitude of α as 10^{-6} for ions in most gases. An early theory for the value of α, deduced by Langevin, was based on assumptions which were erroneous as applied to the usual conditions met with in a gas. This, together with the complicated nature of the processes at work and the experimental limitations, imposed thwarted interpretation for many years. In general there are four possible steps in the recombination process: a diffusive approach, an active electrostatic attraction, a mutual orbital encounter, and the transfer of charge. Depending on the density of the ions, the nature of the carriers, the pressure, the temperature, and the energy of negative ion formation, some of these four processes either are the determinative factor fixing α or may even be absent. Furthermore, measurement does not yield the concentrations needed directly but gives the quantity of ions, q, in a given gross volume, V. Use of the values of concentration n determined by the ratio q/V requires, then, a random but isotropic distribution of ions in space. With some forms of ionizing agencies used such as β particles or γ rays and X-rays, this is not achieved, and n changes in time by diffusion irrespective of the recombination loss, thus complicating theory. This latter aspect is also affected by the speed of attachment of electrons to molecules to give negative ions. Finally, recombination is a slow process, and the time required to get measurable changes in concentration n by recombination is of the order of 10^{-2} second or more long. Thus, loss by diffusion of ions and especially of electrons to the walls causes trouble. The worst difficulty, however, lies in the fact that in all but the purest gases made of single types of atoms the ions change their size and character during the 10^{-2} second of measurement, thus altering α continually. As a result of a long series of investigations beginning in 1925, using the most modern techniques by Marshall (*53*), Luhr (*52*), and finally Gardner (*22*) in Loeb's laboratory, fairly good results were obtained in one gas only (O_2) by Gardner in 1938. Similar techniques were used nearly simultaneously by Sayers (*69*) for air, and while the data are good, the values cannot be compared with theory because of the heterogeneous nature of air. Even more than for ionic mobilities are the values of the coefficient of recombination dependent on ionizing conditions, gaseous purity, and composition, so that practically all data given in the literature are unreliable. For purposes of calculation the values of the coefficient given by Gardner and Sayers may be used as orienting magnitudes for ion-forming gases. The coefficient of recombination for *electrons* and positive ions cannot be evaluated in gases under normal conditions because of its low value in contrast to rapid electron diffusion. Kenty (*35*) and Mohler (*58*) and associates have evaluated it for arc plasma and obtained values of the order of 2×10^{-10}.

which is higher than wave mechanical theory yields but very small compared to the values of about 2×10^{-6} for air and O_2. Later work of Sayers on arcs and of Craggs on spark channels have confirmed and extended these findings.

In theory the mechanism of recombination depends on the relative importance of the four steps indicated. It thus depends on the distribution of ion pairs in space, the nature of the carriers, the pressure, the temperature, and the density of ionization in a complicated fashion. Thus, much confusion has existed for a long time. Lately Jaffé (*31*) has studied the general problem and indicates that for very high gaseous densities the original Langevin theory applies. From some 10 atmospheres down to 2 atmospheres, a diffusion theory of Harper (*24*) which, except for an undefined constant, is in form the same as Langevin's equation, is valid. Thomson's (*78*) equation is applicable and has been experimentally established to hold from about 1 atmosphere down to 10^{-2} mm. pressure. Below this, a simple theory proposed by Loeb (*47*) is indicated. For recombination under α-particle ionization, the theory developed by Jaffé (*32*) must be used. No adequate quantitative theory has yet been derived for initial recombination with X-ray ionization for time intervals shorter than 0.1 second.

In the study of gaseous discharges such as arcs or glows with ion concentrations in the order of 10^8 ions/cm.^{3}, much information of value can be obtained by the use of electrical probes. With such electron and ion densities and greater ones, the Maxwellian energy distribution again begins to appear, despite the action of the field. Thus, good approximate values of ion and electron densities, ion and electron energies, space, and wall potentials can be derived. These techniques were initially developed by Langmuir and Mott-Smith (*43*), Compton (*42*), Schottky (*71*), and others. In recent years advances enabling the measurements to be analyzed to give electron energy distributions of non-Maxwellian form have been introduced by Druyvesteyn (*16*). An excellent alternating current method of study of the energy distribution has been developed by Sloane and McGregor (*73*) in the laboratory of Emmeléus at Belfast.

Probably the greatest contribution to the fundamental analysis of electrical discharge mechanisms is due to the studies of Townsend (*81*) and his group, which began in 1899 to 1903 on the two ionization coefficients. Townsend observed that if one started a current of i_0 electrons from the cathode in a uniform field plane parallel gap of length x and placed a high field, X, at a pressure, p, across the gap, the current of electrons and ions, i, measured after the initial electrons had crossed the gap was given by $i = i_0 e^{\alpha x}$. Here α is a constant dependent of the nature of the gas and the value of X/p. It gives the average number of new electrons produced per unit distance traversed in the field direction by one electron through ionization by collision of the gas atoms or molecules. Townsend further found that this quantity, α, now designated as the first Townsend coefficient, varied such that one could write $\alpha/P = f(X/p)$. The $f(X/p)$ is a complicated slanting S-shaped curve in which the *initial* rise was shown by Masch (*54*) in Aachen and Sanders (*68*) in Loeb's laboratory independently to be of the form $e^{BX/p}$, while the top of the curve has a form varying about as $(X/p)^{1/2}$, as shown by Posin (*64*). Although earlier evaluations suffer from deficient control of gaseous purity, the later results of Penning and Kruithoff (*39*) in Eindhoven on the inert gases and of Bowls (*6*) and Hale (*23*) in Loeb's laboratory for pure N_2 and H_2 probably begin to represent the proper values. The work of both Penning and Loeb's group demonstrates the effectiveness of the actions of certain impurities present to 1 part in 10^5 or 10^6. While earlier theoretical attempts to evaluate α before a proper appreciation of interaction between electrons and atoms or molecules was at hand are faulty, we now have a proper procedure outlined in the theory first suggested by Holm (*27*) but finally developed by Emmeléus, Lunt, and Meek (*18*). It cannot today be applied to calculations of α/p except in limited regions where the electrons have the Maxwellian energy distribution, in that it depends critically on the form of the distribution law. One can then rest assured that the pioneering work done by Townsend is basically correct and feel quite confident in the applications of the first function. Aside from the ignorance of the correct electron energy distribution, the information most urgently needed today by the engineer is a reliable set of experimentally determined values of α/p over large ranges of X/p in important gases of recognized purity. The need for such data is evident when it is noted that we have as yet no values for α/p in pure mercury-free air from X/p 20 to X/p 1,000, the region covering most discharge phenomena in air. Values in mercury-contaminated air may well be 25 per cent off at high values of X/p.

Townsend's second (*83*) great contribution in the study of discharges lay in the analysis of the curves for i at higher values of X/p, in air above X/p = 100, at which point i increases more rapidly than according to $i = i_0 e^{\alpha x}$. This is seen at once by plotting $\log \frac{i}{i_0}$ against x. The resulting linear graphs, whose slope gives α, are replaced by lines that are initially straight but curve upward at higher values of x. This upcurving was ascribed by Townsend to the creation of β new electrons, by impact of *positive* ions on molecules, per unit distance of advance from anode to cathode. Both Townsend and Thomson (*76*) in about 1902 independently recognized, however, that the effect of this β mechanism could also be produced if each positive ion striking the cathode had a chance, γ, of liberating a new electron from the cathode.

While Townsend has for a long time adhered to his

riginal assumption that positive ions can ionize in the as, the extensive experimental data and theoretical udy have shown that, with values of X/p existing in he gas, positive ions cannot ionize (*89*). Meanwhile had been shown by Brode and Neumann (*10*) that ther secondary mechanisms beside the electron libera- ion can be effective in producing the observed increase n the curves of Townsend. These mechanisms are photo- lectric liberation at the cathode originally proposed by aylor (*75*), Holst (*29*), and others, the action of meta- table atoms on the cathode (*36*), and photoelectric ction in the gas, which plays important roles in gaseous reakdown at higher pressure (*14*) but is probably of ittle significance under Townsend's experimental condi- ions. Aside from the quantity, β, then, at least three rocesses, electron liberation by positive ion bombard- ment, photoelectric liberation from the cathode, and, in ppropriate gases, action of metastable atoms at the athode, are known by experiment to give adequate umbers of electrons to yield a satisfactory process. t now happens that the extent of the curved portions f Townsend's curves are not adequate to enable one o differentiate the processes active, and Brode and Neumann in 1928 showed that within the limits of un- ertainty each of these processes would yield an equation or i, with appropriate constants that would fit the ob- served curves. In consequence we will designate what Townsend originally called his coefficient β as the second Townsend coefficient without implying any specific mechanism. It is conventionally ascribed today to a liberation of electrons by positive ion impact on the cathode and designated by the Greek letter γ. The resulting current, i, for high values of X/p is thus given by $i = \frac{i_0 e^{\alpha x}}{1-\gamma e^{\alpha x}}$. If the other processes are active, equations of analogous form with appropriately altered constants can be used.

The essential validity of these conclusions is again indicated by the results obtained by Bowls and Hale in pure N_2 and H_2, using different cathode surfaces, once mercury contamination had been eliminated, as well as by the work of Huxford and Engstrom (*20*).

While the recent advances have clarified the situation and indicated the solution, the state of knowledge is woefully deficient, for in the interpretations of most discharge phenomena the values of these various second- ary mechanisms are indispensable. The current needs in this direction will now be indicated.

While the beautiful studies of Oliphant (*62*), the 37th Kelvin lecturer, and others (*33*) on the secondary electron liberation by positive ion impact gave data for ions of higher velocities, nothing is known about the efficiency of different ions on various surfaces for ions below 20 volts of energy. Nor is the relation between the work function of the cathode and this liberation known. Such defects in knowledge extend as well to the liberation of electrons by metastable atoms. Likewise, while the work of Taylor and others has indicated the order of magnitude of the photoelectric yields from the cathode by photons produced in a Townsend gap, there is abso- lutely no proper quantitative data either on the actual yields for different metals under different conditions of gaseous content or on the changes in work functions of such surfaces with different gas coatings. There is also little information as to the efficiency of the various wave lengths in the ultraviolet below 1,000 A. either at surfaces or in gases, or of the absorption coefficients of such photons in the gases used.

Finally, we are in complete ignorance of the yield of photons accompanying the electron avalanches caused by Townsend's α in their passage to the anode at various electron energies in fields near those of breakdown. This deficiency is now more serious, since the photon produc- tion is essential for the streamer process which charac- terizes all breakdown at higher pressures and gap lengths. Hence, intensive study of photon production, absorption, and photoionization in standard gases constitutes one of the most urgent fields of investigation. It is believed that the postwar experimental techniques are probably now adequate for the tasks indicated above.

The original criterion for the breakdown of a plane parallel gap to yield a spark, which Townsend (*82*) took from the equation $i = \frac{i_0 e^{\alpha x}}{1-\gamma e^{\alpha x}}$ by setting $1 - \gamma e^{\alpha x} = 0$, has been substantiated in fact. It is now established that Townsend's use of the equation above for this purpose was unwarranted, but Holst (*29*) has indicated that the condition fixes the *threshold* for a spark breakdown on other considerations. This correct interpretation, as more recently indicated by Loeb (*49*), at once leads to an understanding of the statistical fluctuation in the values of the potential at which individual sparks appear and further correlates well with recent time-lag studies. Experimental confirmation of the Townsend-Holst mechansim for sparks at relatively low pressure and gap length has been achieved by the time-lag studies of Schade (*70*). As, however, Meek (*57*) has indicated in his excellent paper before the Institution, after 1927 many lines of evidence converged to show that the mechanism as pictured by Townsend and Schade for the plane parallel gaps at lower pressures cannot apply at pressures approaching atmospheric and gaps in excess of a few millimeters in length. The work of Loeb (*85*) and his students on positive corona, beginning in 1936, inevitably led to the development of the streamer theory of the spark by 1940. For this Meek (*56*) provided the quantitative criteria. Slightly later and from quite a different line of investigation using a C. T. R. Wilson Cloud Chamber, Raether (*65*) in Jena arrived at the streamer mechanism of breakdown and actually photo- graphed the streamers. Quite independently of Meek, he arrived also at the same quantitative criterion, al-

though he did not carry his investigations very far. The logical extensions of this new mechanism were carried as far as speculation and the war permitted by Meek and Loeb in their book (*51*) on the mechanism of the spark. This analysis leads to countless unanswered questions, and the problems awaiting solution are too many to mention. The famous Paschen law that the sparking potential is a function of the product of pressure and gap length is not obeyed by Meek's theory, and the recent measurements of Howell (*30*) indicate that the discrepancies are very large at higher pressures. There are no continuous, consistent sets of data on sparking potentials in mercury-free dry air with pressure varying and gap constant, and vice versa, over a sufficient range of pressure and gap length values to permit of a test of the theory. Such an investigation by one laboratory is urgently required but is an exceedingly laborious and perhaps costly undertaking, as high potentials and fairly long gaps free from electrical field distortion are needed. Another urgent study requires that the points of transition from the Townsend mechanism to the streamer mechansim at different pressures be determined. Present estimates range from a value of 200 mm. pressure × cm. gap length for a 1-cm. gap in air to 1,000 mm. × cm. Other problems involve the study of the streamer mechanism of breakdown which proceeds from a negative point, and the question of the sparking potential for very long gaps.

The use of the simultaneous application of all available methods of study as initiated by Loeb's (*50*) group and the development of new, fast oscillographs have led to notable advances in the understanding of the positive and negative corona mechanisms and their ultimate breakdown to a spark. Much still remains to be done, but progress appears to be satisfactory. The study of spark breakdown with alternating potentials, particularly as functions of frequency, has shown that the threshold is lowered some 17 per cent as frequency is increased from 10^4 to some 4×10^5 cycles in short gaps, doubtless owing to positive space charge accumulations (*67*). As frequencies go up to the radar range of 10^{10} to 3×10^{10} cycles, the potential rises some 25 per cent above the d-c breakdown. In this case the electrons travel only short distances between reversals of the field, and the whole mechanism is little understood. The investigation of the whole range of frequencies and a study of the mechanisms at work present a series of fascinating problems for an interested observer.

Considerable advance in the understanding of the vacuum spark, so important in modern industrial application, has resulted from Bennett's (*4*) theory of the self-focusing beam. This theory materially assisted the successful study of the mechanism of the vacuum switch by Koller (*37*) in Loeb's laboratory.

One or two comments are in order with respect to glow discharge and the arc. Thanks to the development of probe studies, more is known about the gl than any other form of gaseous discharge. Thus, where probe studies are possible, gradients, potentials, electr energies, and, thanks to Druyvesteyn (*16*) and Sloa and McGregor (*73*), their energy distributions can measured. There are, however, two regions in whi such methods are not too successful. These are t cathode dark space, where ionization is insufficien and the region of the anode fall of potential. It is desirab that much more data be gathered concerning the ano fall where the electrons have high energies, a negati space charge sheath exists, and in which the positi ions are created. The vital element in any quantitati treatment of the glow discharge lies in a knowledge the cathode fall of potential. Since probes fail in th region, only one proper survey has been made in o discharge type by the late F. W. Aston (*2*), using t deflection of a fine cathode-ray beam. It is urgent tha further studies be carried out in other gases over a rang of pressures. With modern improvements in techniques such as electron multipliers and cathode-ray oscillo graphs, it is expected that more information can b obtained by electron beam measurements. Recen investigations by Morton (*60*) and later work by Johnso (*34*) in Loeb's laboratory have shown that in field which vary as rapidly with distance as do the dark spac fields, one cannot apply the first Townsend coefficien as measured in uniform fields for calculating the ioniza tion produced. Not only is the ionization from 2 to or more times greater than given by the integration o such coefficients over the cathode fall, but the spatia distribution of ionization about the cathode is just th inverse of that given by the use of Townsend's functions. This recent investigation explains the striking charac teristic structure of dark space and glow always observed and previously at variance with theoretical prediction on Townsend's function Thus, past attempts at calcula tion of the glow discharge using the condition of Holst and Oosterhuis (*28*), Seeliger (*72*), and v. Engel and Steenbeck (*19*), that $\gamma e^{\int \alpha x} = 1$, must be modified by replacing $e^{\int a\,dx}$ by the evaluations of G. W. Johnson for the conditions found by Morton. Johnson's work covers these functions for pure H_2 and mercury-free air from 0.1 to 760 mm. pressure. More data such as Johnson's on other standard gases are urgently needed, and the investigation should be extended to spherical electrode symmetry instead of cylindrical.

With regard to the mechanisms of arcs, the high temperatures and microscopic scales of distance involved in the critical areas preclude study, and much remains to be done. While the refractory electrode arcs are amenable to some measure of theoretical treatment, the mechanism of the low-melting-point metal vapor arc is not clearly understood and is not amenable to solution. There seems little doubt but that the source of

ns in such arcs is in the plasma of the gas adjacent the cathode and previous speculations such as those voking field emission are inadequate to account for the henomena. Equally illusory are the attempts to account r the mechanism by the glib term, "temperature nization." Granted that the region near the cathode at a high temperature, conditions are so far removed om equilibrium that one cannot apply M N. Saha's quation. It appears that we are confronted in this tudy with a situation presently also being encountered ear the surface of particles moving with supersonic elocities, in which we cannot apply the familiar procedures of thermodynamics and kinetic theory. In these ases one has in microscopic regions large numbers of toms or ions moving to the cathode or a surface, interchanging energy with solid or liquid surfaces, and leaving hose surfaces with considerable directed momentum nd energy. In these regions there are thus large numbers f particles of energy many times those of thermal quilibrium with directed velocities. The conditions or ionization are present, but a new mathematical hysics is needed to calculate the consequences which epart widely from equilibrium conditions.

In presenting this summary I have attempted to nclude the advances and accomplishments in the last 0 years. What I consider more important, however, n this brief summary is not so much a glorification f the past but an indication of the problems still to be olved. It is my hope that the indications of just a few f these will arouse an interest in this presently neglected ield, inspiring some few individuals, at least, to aid physicist and engineer in the work which remains to be done.

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The Truman Veto

Howard A. Meyerho[illegible]
Department of Geology, Smith College, Northampton, Massachuse[illegible]

SCIENTISTS WILL UNDOUBTEDLY BE tempted to condemn the President for vetoing the National Science Foundation Act of 1947 (S. 526), and some have already yielded to this temptation. It may be hoped that the rest will apply the same processes of analytical thought to the reasons behind the veto as they conscientiously apply to their own professional problems.

Although the vetoed Smith bill has evolved a long way from the Magnuson bill, which was introduced into the 79th Congress during the summer of 1945, it is basically the same type of legislation. It has made some political concessions on patent provisions; it has prescribed a feeble measure of geographical distribution for scholarships; and it has liberalized the divisional structure of the Foundation. However, it strictly adheres to administration by a part-time board of eminent scientists, and it commits the legislative blunder of creating an Inter-Departmental Committee on Science whose chairman is an appointee of the part-time board, notwithstanding the fact that his powers affect many of the executive departments of the Government. For the President to have no say in naming a committee chairman who, in regard to scientific matters, outranks his own Cabinet officers in the executive branch of the Government is little short of preposterous. As originally drawn and finally passed, the bill still precludes reasonable geographic distribution of funds for research. It is, in short, the Bush bill, based upon the Bush report, *Science, the endless frontier*. It was, moreover, engineered through committee by the man who, in 1945 and 1946, personally represented Dr. Bush.

In 1946 the Administration made it perfectly clear that, if such a bill reached the President's desk, it would be vetoed, and the reasons given were substantially the same as those contained in the veto message. That the Administration's views were shared by many scientists and other educators was evident from the size and vigor of the committee formed by Harlow Shapley and Harold C. Urey. Furthermore, that the President had not changed his mind in 1947 must have been clear to Dr. Bush and the other supporters of the bill. It is reported tha[illegible] Senator Smith had even more authoritative informatio[illegible] regarding the President's attitude.

In the face of the President's forthright statement[illegible] S. 526 was drafted and introduced. Little regard wa[illegible] paid to the carefully considered recommendations of th[illegible] Inter-Society Committee, sponsored by the America[illegible] Association for the Advancement of Science. The Mors[illegible] Amendment, which liberalized the geographic distribu[illegible] tion of research funds and which might possibly have[illegible] saved the bill from veto, was defeated in the House. S[illegible] the Bush bill went to the President, and the President[illegible] kept his word. If the proponents found no reason to[illegible] change the bill, certainly the President had no reason to[illegible] change his mind. Indeed, it was virtually necessary to[illegible] veto a piece of legislation which assigned powers in the[illegible] executive branch of the Government to a director whom[illegible] the President could neither name nor remove.

Scientists must look at facts squarely and honestly. There are many who favor S. 526 because they have faith in administration by scientists who have demonstrated ability in science and in administration. But there are as many more who feel that, however capable the administration of a scientific board may be, the Board is bound to consist of men who, from temperament and experience, will administer Government-supported scientific research in the same way as industrial and national defense research. At present fewer than 50 institutions are receiving well over 90 per cent of all research financing from industry and from the Army and Navy. There is no complaint about this allotment of funds, but there is serious objection to the disproportionate award of additional grants to the same institutions. Not only will it widen the gap between the large and the small universities and technical schools; it will literally dwarf the latter by drawing Foundation-subsidized students and underpaid but competent instructors to the former.

Two provisions of the bill make such a trend inevitable: (1) the qualifications set for members of the Board, who will necessarily be mainly big-name men from big-name institutions; and (2) the stipulation that research grants

hall be made to the institutions best qualified to carry ut the projects in question. Such a policy makes good ense to practical business-minded men, but its execution ill serve to deepen the existing foundations of science ather than to broaden them or to build new ones.

The administrative structure of the Foundation as proposed in the vetoed bill would have concentrated too much power in the hands of too few, without a single effective control from scientists, the Congress, or the President. Let us grant that the choice of scientists would fall upon men in whom there is complete public and professional confidence. The full Board is required to assemble but once a year, and effective control rests with an executive committee of 9. Everyone will agree that the advice and guidance of such men is imperative to the success of a Foundation; yet, regardless of the character of the men who will be selected, and who will undoubtedly give sincere and disinterested service to the public and to science, is it wise to leave American scientists without recourse or appeal from the acts of the Foundation, save through the slow machinery of presidential appointment or through violent death by the withholding of appropriations?

Already Government-financed research and not a little industrial research are guided and, to some extent, controlled by a small group of scientists. Let it be said they have done their job well. But many scientists are as deeply concerned about close professional control as they are about the hazards of political control. If the Smith bill (S. 526) assures freedom from the latter, it certainly invites and almost prescribes professional control, which may quite conceivably be identified with the group directing so much industrial and military research. That there may be something to fear is suggested by the way in which S. 526 was introduced and maneuvered in Congress, the limited sources from which advice was sought, and the studied disregard of preferences expressed by the Inter-Society Committee, which had the broadest possible base among U. S. scientists.

There are those who sincerely believe that no such danger exists, but this belief is not held by many who were close to the situation in Washington. It certainly was not held by President Truman, who has persistently shown a genuine concern about the welfare of science. Is it not reasonable, now, to urge that those who have tried to get a particular type of legislation passed and have twice failed, relinquish the task to disinterested, scientists, who will view the problem more broadly and dispassionately? There are many such men, and there is no hostility toward science either in the Congress or in the White House. The Senate has, by passing S. 1850 in 1946 and S. 526 in 1947, demonstrated that it is trying to give scientists what they want. What it needs now is the advice of representatives of science rather than advocates of a partisan point of view. Surely the Association's Inter-Society Committee comes closer to the requirements of the present situation than a group determined to give science what is supposedly good for it, whether it wants it or not. This is the time for leaders in the Inter-Society Committee to take over and to act.

NEWS *and Notes*

Since many scientists will not have seen the complete text of President Truman's Memorandum of Disapproval of S. 526, dated August 6, and since many others will undoubtedly wish to refer to it in the coming months, the veto message is presented in full below:

I am withholding my approval of S. 526, the National Science Foundation Bill.

I take this action with deep regret. On several occasions, I have urged the Congress to enact legislation to establish a National Science Foundation. Our national security and welfare require that we give direct support to basic scientific research and take steps to increase the number of trained scientists. I had hoped earnestly that the Congress would enact a bill to establish a suitable agency to stimulate and correlate the activities of the Government directed toward these ends.

However, this bill contains provisions which represent such a marked departure from sound principles for the administration of public affairs that I cannot give it my approval. It would, in effect, vest the determination of vital national policies, the expenditure of large public funds, and the administration of important governmental functions in a group of individuals who would be essentially private citizens. The proposed National Science Foundation would be divorced from control by the people to an extent that implies a distinct lack of faith in democratic processes.

Moreover, the organization prescribed in the bill is so complex and unwieldy that there is grave danger that it would impede rather than promote the Government's efforts to encourage scientific research. The Government's expenditures for scientific research and development activities currently amount to hundreds of millions of dollars a year. Under present world conditions this work is vital to our national welfare and security. We cannot afford to jeopardize it by imposing upon it an organization so likely to prove unworkable.

Under S. 526, the powers of the proposed Foundation would be vested in 24 members appointed by the President by and with the advice and consent of the Senate. These members would be part-time officials, required to meet only once each year. This group would, in turn, select biennially from among its 24 members an executive committee of 9 members and would exercise its powers through the executive committee. This 9-member executive committee would also be a part-time body required to meet only 6 times a year.

The Foundation would have a chief executive officer, known as the Director.

He would be appointed by the 9-member executive committee unless the 24-member body itself chose to appoint him. The power and duties of the Director would be prescribed by the executive committee and exercised under its supervision.

There would be within the Foundation a number of divisions, each exercising such duties and performing such functions as the Foundation prescribed. There would be no limit upon the number of divisions which the Foundation could establish. For each division there would be a divisional committee. In the case of the Committee for the Division of National Defense there would be a limit of 36 members, half of whom would be appointed by the Foundation and half of whom would be representatives of the armed services. In other cases, there would be no limit upon the number of members of each divisional committee and all of the members would be appointed by the Foundation. Not only would these part-time committees furnish advice and make recommendations concerning the Government's scientific research program, but each divisional committee would also "exercise and perform the powers and duties of its division."

The Foundation would also be empowered to appoint commissions in various fields of research. Three such commissions are specified in the bill, and the Foundation could appoint as many additional special commissions as it saw fit. Each such commission would consist of 6 eminent scientists and 5 members from the general public. After making a survey of public and private research already being carried on, each of these commissions would recommend a research program within its field and constantly review the manner in which such a program was being carried out.

Apart from the conflicts and confusion which would result from this complex organization, the bill would violate basic principles which make for responsible government.

The Constitution places upon the President the responsibility for seeing that the laws are faithfully executed. In the administration of this law, however, he would be deprived of effective means for discharging his constitutional responsibility.

Full governmental authority and responsibility would be placed in 24 part-time officers whom the President could not effectively hold responsible for proper administration. Neither could the Director be held responsible by the President, for he would be the appointee of the Foundation and would be insulated from the President by two layers of part-time boards. In the case of the divisions and special commissions, the lack of accountability would be even more aggravated.

The members of the Foundation would also be authorized to appoint the full-time administrative head of an important agency in the executive branch of the Government, as well as more than 70 additional part-time officials in whom important governmental powers would be vested. This represents a substantial denial of the President's appointing power, as well as an impairment of his ability to see that the laws are faithfully executed.

The ability of the President to meet his constitutional responsibility would be further impaired by the provisions of the bill which would establish an Interdepartmental Committee on Science. The members of this committee would be representatives of departments and agencies who are responsible to the President, but its chairman would be the Director of the Foundation. It would be the duty of this committee to correlate data on all Federal scientific research activities and to make recommendations to the President, to the Foundation, and to the other departments and agencies of the Government concerning the performance of their functions in this field. Thus, an officer who is not appointed by the President, and not responsible to him, would be the man primarily charged with the performance of functions which are peculiarly within the scope of the President's duties—that is, the coordination of the work of executive agencies. This is especially unwise when the activities concerned are so intimately related to the national welfare and security.

There are other compelling reasons why control over the administration of this law should not be vested in the part-time members of the Foundation. The Foundation would make grants of Federal funds to support scientific research. The recipients of these grants would be determined in the discretion of the Foundation. The qualifications prescribed in the bill for members of the Foundation would insure that most of them would be individuals employed by institutions or organizations eligible for the grants. Thus, there is created a conflict of interests which would inevitably give rise to suspicions of favoritism regardless of the complete integrity of the members of the Foundation.

It is unfair to individuals asked to accept public office that they should be put in such a vulnerable position. Moreover, colleges and universities and other organizations seeking aid for scientific research deserve the assurance that the manner and extent of their participation in a national program will be determined on a completely impartial and objective basis.

Adherence to the principle that responsibility for the administration of the law should be vested in full-time officers who can be held accountable will not prevent the Government from utilizing with great advantage the services of eminent scientists who are available only for part-time duty. We have ample evidence of the patriotic and unselfish contributions which such citizens can make to the success of governmental programs. The role to be played by such part-time participation, however, is more appropriately one of an advisory nature rather than of full responsibility. In other governmental programs of vast national importance, this method is used to obtain advice and recommendations from impartial experts as well as from parties in interest. There is no reason why such a system cannot be incorporated in legislation establishing a National Science Foundation.

For the reasons I have indicated, I believe that this bill raises basic issues of public policy. There would be no means for insuring responsible administration of the law. If the principles of this bill were extended throughout the Government, the result would be utter chaos. There is no justification in this case for not using sound principles for normal governmental operations. I cannot agree that our traditional, democratic form of government is incapable of properly administering a program for encouraging scientific research and education.

It is unfortunate that this legislation cannot be approved in its present form. The withholding of my signature at this time, however, will not prevent the Government from engaging in the support of scientific research. Research activities are carried on extensively by various executive agencies under existing laws, and would continue to be carried on whether or not this bill became law. The

y funds made available by the Con-
s for expenditure by the Foundation
funds which might be transferred
m other agencies, thereby reducing the
ounts which those other agencies
ld spend for similar purposes. No
ds were made available for the schol-
hips and fellowships authorized in the
. Thus, there would be no immediate
ns which would justify accepting the
s involved in the approval of this
islation.

I am convinced that the long-range
erests of scientific research and educa-
n will be best served by continuing
r efforts to obtain a Science Foundation
e from the vital defects of this bill.
ese defects in the structure of the
oposed Foundation are so fundamental
at it would not be practicable to permit
establishment in this form with the
pe that the defects might be corrected
a later date. We must start with a law
ich is basically sound.

I hope that the Congress will recon-
er this question and enact such a law
ly in its next session.

bout People

Dorwin **Cartwright** has been named
rector of the Research Center for Group
ynamics, Massachusetts Institute of
chnology, succeeding the late **Kurt**
ewin.

Kenneth **E. Caster,** on leave from
e University of Cincinnati for the past
o years while serving as director, De-
rtment of Geology and Paleontology,
niversity of São Paulo, Brazil, in con-
ction with the State Department's pro-
am of cultural cooperation, will ter-
inate his work there at the end of this
ar. He will remain in South America
ntil September 1948, however, in order
complete geological studies supported
a Guggenheim Fellowship. Upon
is return to Cincinnati the Brazilian
leontologists, **Josué Camargo Men-**
es and **Setembrino Petri,** will assist in
e preparation of stratigraphic and pa-
ontologic reports covering some 50,000
ilometers of Brazilian travels.

Rafael **Rodríguez-Molina,** recently
ischarged from the Medical Corps,
.S.A., with the rank of Lt. Colonel, has
een appointed chief of the Medical Serv-
ce, Veterans Administration Center,
an Patricio Hospital, San Juan, Puerto
ico. Dr. Rodríguez-Molina was for-
erly assistant professor of tropical medi-
cine, University of Puerto Rico and Columbia University.

Gordon H. Pritham, head, Department of Chemistry, University of Scranton, has been named associate professor of physiological chemistry, Department of Agricultural and Biological Chemistry, Pennsylvania State College, and **Eva Ruth Hartzler,** associate nutritionist, University of Hawaii, has accepted a position in vitamin research in the same Department.

Arthur C. Bevan, state geologist of Virginia, Charlottesville, and chairman, Division of Geology and Geography, National Research Council, has been appointed principal geologist in charge of the Geological Resources Section, Illinois Geological Survey, Urbana. Dr. Bevan succeeds **Ralph E. Grim,** who will devote more time to the Survey's research program on clay minerals.

T. G. Blocker, Jr., professor of plastic and maxillofacial surgery, has been appointed director, Post-Graduate Training Program, University of Texas Medical Branch, Galveston.

W. N. Bangham, formerly director, Plant Research Department, Goodyear Rubber Plantation Company, Costa Rica, has been appointed editor of *La Hacienda*, a Spanish journal covering agricultural and allied industrial subjects, with offices at 20 Vesey Street, New York City.

Richard B. Goldschmidt, of the University of California, has returned from New Zealand and Australia, where he has been for the past 6 months. In New Zealand he delivered a large number of lectures under the auspices of the Royal Society, the University of New Zealand, and the Council of Scientific and Industrial Research in all universities and agricultural colleges of the Dominion. In Australia he gave a series of lectures at Sydney University and the Department of Scientific Research in Canberra.

Warren Andrew, professor of histology and embryology, Southwestern Medical College, Dallas, has been appointed professor of anatomy and chairman of the Department, George Washington Medical School, Washington, D. C.

John T. Buchholz, professor of botany, University of Illinois, has been granted a year's leave of absence to collect gymnosperms and other plants in New Caledonia. He will also visit the Fiji Islands, Tasmania, and Australia. Dr. Buchholz may be reached % Biological Station, Noumea, New Caledonia.

John H. Weitz, Pennsylvania Topographic and Geologic Survey, has been appointed assistant professor of geology, Lehigh University.

Emanuel D. Friedman, chairman, Department of Neurology, New York University College of Medicine since 1927, retired September 1 as professor emeritus. Dr. Friedman has been on the College of Medicine staff for 37 years.

Oswald Tippo has been named acting head of the Department of Botany, University of Illinois, succeeding **Neil E. Stevens,** who transferred to the Department of Horticulture, College of Agriculture, on September 1.

Grants and Awards

The Biological Stain Commission has made two grants for research on stains and staining methods: (1) $1,500 (renewal) to Northwestern University Medical School for research in methods of neurological microtechnic under the direction of H. A. Davenport; and (2) $1,800 to McGill University for work in cytochemistry under the direction of C. P. Leblond. The latter work will deal with the Feulgen reaction and the significance of cytological structures giving that reaction.

George C. Supplee, president, G. C. Supplee Research Corporation, Bainbridge, New York, will be the recipient of the 1947 Borden Company prize of $1,000 and a gold medal for research in the chemistry of milk to be presented September 15 at the 112th national meeting of the American Chemical Society in New York. Dr. Supplee, who is credited with developing the first practical methods for commercial irradiation of milk to increase its vitamin D content, will deliver his award address September 18 at a symposium on the biochemistry of milk.

The American Allergy Fund has announced the availability of grants-in-aid for research which will be made to investigators in the biological sciences, both medical and nonmedical, whose problems meet the requirements of the Scientific Advisory Council. Preference will be given problems with immediate relationship to allergy, although investigations in physiology, biochemistry, pharmacology, immunology, genetics, and other basic

sciences are solicited. The grants will be made for one year in amounts not to exceed $3,500 and may be renewed from year to year if continuation is warranted. Applications (7 copies) should be addressed to the American Allergy Fund, 525 Erie Building, Cleveland 15, Ohio, Attention: Scientific Council, and should contain the following information: a statement of specific research problems and an outline of the method or methods of procedure to be followed; a description of research facilities in the institution where investigator will employ the grant; a tentative budget; and a statement of the applicant's research record, accompanied, if possible, by publications or reprints.

The American Allergy Fund, organized a little more than a year ago, has a five-point program embracing the following aims: to finance research in basic sciences; to keep physicians informed of the latest developments in allergy; to encourage medical students in the study of allergy; to promote public education in allergy; and to help provide care for needy allergic children and adults. Jonathan Forman, editor, *Ohio State Medical Journal*, is president of the Board, and Anton J. Carlson, University of Chicago, is chairman of the Scientific Advisory Council.

Colleges and Universities

A Department of Statistics has been established within the Division of Science, Iowa State College. **Raymond J. Jessen,** resident collaborator, Bureau of Agricultural Economics at the College, will be acting head of the new Department and also of the Statistical Laboratory.

The effect on animals and humans of changes from one altitude to another has been studied this summer by members of the Zoology Department, University of Chicago, under the direction of Carl R. Moore, chairman of the Department. These experiments, conducted with four separate animal colonies maintained in the zoology building at the University of Chicago (600 feet), at Idaho Springs, Colorado (7,500 feet), at the summer geological and biological field station of the University of Colorado (9,600 feet), and in the cosmic ray laboratory on the crest of Mt. Evans (14,260 feet), were suggested by earlier work by Carlos Munge, of the University of Peru, who was interested in the problem of altitude sickness in humans and that of sterility induced in certain breeds of sheep imported from low altitudes to pastures above 12,000 feet in the Andes. The results of the experiments should be of interest not only to scientists but also to mountain travelers, pilots, and stock raisers.

Reed College has announced the inauguration of a new course entitled "The Development of the Experimental Sciences," to be given during the fall and spring semesters of 1947–48. The course is described as "an inquiry into the history of the sciences in relation to society and a study of the methods of science in gaining and applying new knowledge." It has been organized by Frank H. Hurley, associate professor of natural science, and is based on a similar, but less extensive course first given during the summer of 1944. The new course is offered at the junior level and is open to all students who have had one or more years of study in the laboratory sciences.

Meetings

An International Conference on the Earth Sciences and Civilization will be held September 21–23 at South Duxbury, Massachusetts, under the auspices of the Commission on Continental and Oceanic Structure, and the Committee on the Social Value of the Earth Sciences, International Union of Geodesy and Geophysics. Members of the latter organization or of the American Geophysical Union who wish to attend should notify R. M. Field, South Duxbury, or J. A. Fleming, 1530 P Street, N. W., Washington, D. C.

The 8th Pittsburgh Conference on Applied Spectroscopy, sponsored by the Spectroscopy Society and the University of Pittsburgh, will be held in the Mellon Institute Auditorium, November 13–15. Sessions on both emission and absorption spectroscopy are planned. Those desiring to submit papers for the Conference should send in the title and a brief abstract by October 4. Inquiries or offers of papers should be addressed to Mary E. Warga, University of Pittsburgh, Pittsburgh 13, Pennsylvania.

The American Academy of Dental Medicine will hold a luncheon meeting prior to the Greater New York Dental Meeting at Hotel Pennsylvania, Sunday, December 7, at 12:30 P.M. A round-table discussion will follow. All members and interested dentists and physicians are invited to attend. Communications should be addressed to Dr. William M. Gr... hut, secretary, 124 East 84th Street, N... York City.

Recent Deaths

George Matthai, of the Zoological La... oratory, Cambridge University, and ... authority on corals, was found dead Ju... 25.

Alexander W. Winkler, 38, assista... professor of internal medicine, Yale U... versity, died June 26 in New Haven Ho... pital.

Elliott Carr Cutler, 59, Moseley pr... fessor of surgery, Harvard University, a... surgeon-in-chief, Peter Bent Brigha... Hospital, Boston, died August 16 at h... home in Brookline, Massachusetts. D... Cutler was known for his work in hea... and thyroid surgery.

Ernst Laqueur, 68, professor of pha... macology, University of Amsterdam, Ho... land, who first isolated testosterone ... 1935, died August 19 while vacationing ... Switzerland. In 1946 Dr. Laqueur r... received the Berzelius Medal, awarded b... the Swedish Medical Society.

Cullen Warner Parmelee, 73, profes... sor emeritus of ceramics engineering, Un... versity of Illinois, died August 20 at h... summer home at Pilgrim, Michigan.

Sir Clive Forster-Cooper, 67, direc... tor, British Museum of Natural History, ... died August 23.

James T. Saldana, 64, professor ... physics, St. John's University, Brooklyn, ... for 27 years, died August 23 after a lon... illness.

Clark Wissler, 76, curator emeritus ... Department of Anthropology, American ... Museum of Natural History, died August ... 24 after a brief illness. Dr. Wissler ha... served on the Museum staff since 1902.

Just 13 months after receiving the first shipment of a pile-produced isotope from the Clinton laboratories, E. V... Cowdry, of the Barnard Free Skin and ... Cancer Hospital, St. Louis, had the privi... lege of announcing to the delegates assem... bled for the Fourth International Cancer ... Congress in St. Louis the Government's ... decision to embark on a program of distri... bution of radioactive isotopes to foreign ... research centers and hospitals. The

tems listed as available are restricted o the following: antimony 122, 124, 125; rgon 37; arsenic 76, 77; bromine 82; cal- ium 45; carbon 14; chlorine 36; cobalt 60; opper 64; gold 198, 199; iodine 131; iron 5, 59; mercury 197, 203, 205; phosphorus 2; potassium 42; silver 108, 110, 111; trontium 89, sulfur 35; sodium 24; and inc 65, 69. Conditions under which for- gn shipments may be made include ap- roval by the Commission prior to ship- ent in the manner followed with domestic equests; the making of progress reports very 6 months; and the opening of lab- ratories in which the isotopes are used to ualified scientists irrespective of nation- lity.

The total destruction of the rchives and library of the Experimental tation for Sugar Cane at Pasuruan, East ava, by order of the Indonesian Repub- can authorities has been reported by the overnment Botanic Gardens, Buiten- org, Java, to Frans Verdoorn, editor of *hronica Botanica*. This planned destruc- ion of detailed data on hybrid cane ecies, plant diseases and resistant strains, st plantings, soil conservation, and irri- ation is an immeasurable loss to the ience of sugar cane cultivation and proc- ssing in general and to the Java sugar dustry in particular.

lake Plans for—

American Chemical Society, Sep- mber 15–19, New York City.

Clinical Conference on Diagnosis nd Treatment of Poliomyelitis, Sep- mber 15–17, Warm Springs, Georgia.

American Geophysical Union, 21st egional Meeting, September 17–20, Vesleyan University, Woods Hole Ocean- graphic Institution, and Harvard Uni- ersity.

American Institute of Electrical Engineers, Middle Eastern District Meeting, September 23–25, Dayton, Ohio.

Oregon Geography Council, fall con- erence, October 4, Oregon State College, Corvallis.

American Association for the Advancement of Science, 114th Meeting, December 26–31, Chi- cago, Illinois.

COMMENTS
by Readers

We wish to register a protest against two recent articles by Representative J. Parnell Thomas of New Jersey, chairman of the Congressional Committee on Un-American Activities. These are: "Russia Grabs Our Inventions" (*American Magazine*, June) and "Reds in Our Atom Plants" (*Liberty*, June 21).

Mr. Thomas' articles are inaccurate to the point of absurdity. Thus, in one, he says that the National Bureau of Standards tests patents, which will be news to the Bureau and the Patent Office. Nevertheless, his attacks on the patriotism and honesty of American scientists are bound to be taken seriously by many people and will do harm.

We are particularly aroused at the treatment accorded Dr. Edward U. Condon, director of the Bureau of Standards, who, by inference and innuendo, is made to appear engaged in reprehensible and subversive activities.

Dr. Condon is a distinguished scientist, the director of a great national institution, a former president of the American Physical Society, sometime adviser to the Senate on matters concerning atomic energy, and a former associate director of the Westinghouse Laboratories. By no stretch of the imagination is he a Communist or an unloyal American.

Mr. Thomas especially attacks Dr. Condon for his membership on the board of directors of the American-Soviet Science Society. This organization, which has received a grant from the Rockefeller Foundation, is in no way connected with any pro-Soviet agency. It exists solely for the purpose of furthering international exchange of scientific information (something scientists everywhere favor), particularly by making Russian scientific papers available in English.

Mr. Thomas furthermore plainly implies that Dr. Condon is dodging testifying before the Thomas Committee. Dr. Condon, however, can hardly be an unwilling witness, since he has never been invited to testify, although he was visited in March by two of the Committee's investigators, with whom he cooperated completely.

Mr. Thomas owes Dr. Condon and the Nation a retraction and an apology for his innuendoes concerning a distinguished scientist. (IRVING LANGMUIR, D. A. MACINNES, GEORGE B. PEGRAM, I. I. RABI, W. M. STANLEY, and HAROLD C. UREY.)

Bacteriostasis is an important phase in the recent development of chemotherapy, and the quest for new natural and synthetic agents will, no doubt, pursue its course (H. W. Florey. *Brit. med. Bull.*, 1946, **4,** 248; V. C. Barry. *Nature, Lond.*, 1946, **158,** 863). The factors underlying bacterial inhibition extend, however, beyond the interaction of some specific substances with normal metabolic products of certain bacteria. Thus, in the absence of regulated administration, the bacteriostatic treatment itself may evoke in bacteria an environmental adaptation or an acquired resistance, as manifested by functional (p-aminobenzoic acid, enzymes) and structural (genes, chromosomes) changes (F. R. Selbie. *Brit. med. Bull.*, 1946, **4,** 267).

Again, the physical aspect of this problem is not without consequence. The fatty covering of some bacteria, as in the case of *tubercle bacilli*, may prevent contact between the organism and the agent, which is essential to bacteriostasis. These protective coverings could probably be removed and interaction made possible by the addition of a suitable solvent, such as acetamide, to the solution or colloidal suspension of the agent. Acetamide has a high solvent power, and both it and its hydrolytic products are nontoxic. Furthermore, its constitutional affinity to antibacterial substances, such as allantoin, urea (M. Copisarow. *Chem. Ind.*, 1942, **61,** 67), and sulfonamide, suggests the possibility of its synergic action in bacteriostasis. This may ultimately lead to the reassessment of the effective range of the bacteriostatic agents. (MAURICE COPISAROW, *1 Gildridge Road, Manchester, England.*)

TECHNICAL PAPERS

One Unconsidered Form of the Part Played by the Nervous System in the Development of Disease

S. I. FRANKSTEIN

Institute of General and Experimental Pathology, Academy of Medical Sciences of the USSR, Moscow

It is readily observed that, in walking, flexure of one of the anterior limbs of an animal is accompanied by a simultaneous flexure of the crossed posterior limb; at the same time both remaining limbs are extended and serve as a support to the body. In man, similar relations between upper and lower limbs are maintained in the form of additional movements of hands in walking.

It is known that these movements are made automatically, *i.e.* unconsciously. This is easily shown by removal of an animal's brain using the method of the prominent English physiologist, Sherrington, *i.e.* at the level between the anterior and posterior corpora quadrigemina. Such an animal cannot move independently. Its legs are extended like sticks and are in a state of a sharply marked extensor stress. The flexor response of one of the anterior limbs, produced by electrical or mechanical stimulation, is followed by a simultaneous relaxation in the crossed posterior limb like that observed in normal walking. In the remaining limbs the stress is increased. Marie et Foix showed that these regularities, stated by Sherrington, may also be revealed in men in cases of affection of the nervous system.

We studied the reaction of the limbs to the inflammatory process in animals (cats) decerebrated according to Sherrington's method. The process was evoked by subcutaneous injection of turpentine into the foot. It was found that under the action of the inflammatory process the extensor stress of the limb of the decerebrated animal disappears, and it becomes flaccid. If the inflammatory process spreads not only upon the foot but also upon the leg and thigh, the limb remains flaccid. If the inflammation is localized, instead of an extensor stress there is soon developed a stress in the flexor muscles—the leg is in the flexor posture.

It is curious that inflammation of the limb, as well as its electrical and mechanical stimulation, is followed by a change of the stress in the remaining unaffected limbs. Still more important is the fact that if the inflammatory process evoked in the animal before decerebration was already healed and the animal moved normally, decerebration made soon after healing produced changes of stress in the limbs similar to those observed in cases where the process of inflammation was at its height. The obtained evidence enables us to draw a series of conclusions which are so much the more important because inflammation is one of the most frequent reactions of the tissues of the organism to various injuries, and it is by inflammation that the overwhelming majority of affections is manifested.

(1) Inflammation different from mechanical or electrical stimulation causes *prolonged and persistent* excitation in the nervous system. Instead of the transient changes of stress in the legs of decerebrated animals which is caused by electrical or mechanical stimulation, a persistent change of the tonus is observed. Traces of this excitation may be detected in the nervous system also, some time after healing of the process at the periphery.

This prolonged stimulation produced by the inflammatory focus explains the fact that *the affected limb may maintain an unusual posture for a long time.* While interpreting this reaction in a wide sense as a psychologically comprehensible tendency to take care of the injured organ, we do not pay attention to the fact that in a healthy state it is impossible to maintain the organ in a forced position for a long time—the sense of fatigue comes very soon.

The automatic character of this reaction is also evidenced by the fact that it occurs in decerebrated animals as well.

(2) Stimulation produced by the inflammatory process as well as electrical or mechanical stimulations causes reaction not only in the injured limb but also in other concomitant limbs.

In the normal, uninjured nervous system this additional part of reaction remains latent, being compensated by the nervous system. This has not been taken into consideration by the modern pathology. Meanwhile, the "restricted," "local" character of pathological reactions is the result of the enormous work of the organism in concealing additional nonmanifested components of the reaction. Without such a compensation, *inflammation in the hand should unavoidably result in limping.*

(3) The organism is forced to compensate, to conceal also *the traces* of excitation left by the inflammatory process in the nervous system. As shown above, decerebration of the animal performed soon after healing of the pathological process disturbs this compensation, and, in spite of the healing of the injury, the leg takes the posture which earlier had been due to inflammation. Thus, the recovery of the animal, the restoring of normal function of the injured limb, is determined not only by *healing of the tissue injury*, as is assumed now in general pathology, but also by *compensation of traces of the pathological excitation in the nervous system.*

Thus, the above data reveal a new form of participation of the nervous system in the pathological process as well as in the process of recovery. A number of particular conclusions drawn from these experiments concerning alterations of the predilection type of the posture of limbs in central paralysis and the mechanism of reflex and fixation contractions are reported in another paper.

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Experimental Studies With the Agent of the Common Cold

MORRIS POLLARD and COLEMAN D. CAPLOVITZ

The University of Texas Medical Branch, Galveston

Nasal washings were collected from several cases of common cold infection within 24 hours after the onset of symptoms. The diluent was 2 per cent normal rabbit serum in physiological saline. Each of three such specimens was treated with penicillin sodium (30,000 units for 5 minutes) and then inoculated into the chorioallantoic cavity of 7-day-old incubating chick embryos (0.50 cc./egg). The inoculated embryos were further incubated at 35° C. At 4-day intervals the chorioallantoic fluid was collected from these eggs and pooled from each strain passage. Each strain was passaged blindly at 4-day intervals. Infectivity of the chorioallantoic fluid was checked periodically by inoculating this material into human volunteers[1] by the intranasal route. At no time could visible organisms be cultivated from this material when inoculated into synthetic medium. The results obtained from these three strains were as follows:

Strain I (Haltmar) was negative when chorioallantoic fluid of the second passage was inoculated into two volunteers.

TABLE 1

INTRANASAL INOCULATION OF HUMAN VOLUNTEERS WITH CHICK EMBRYO PROPAGATED COLD AGENT

No	Inoculum	Hours	Symptoms: "Stuffy"	Sneezing	Swollen mucosa	Exudate	Headache	Cough
11	MP strain, 2nd passage	24	−	−	+	−	−	−
		48	++	−	++	++MP*	++	−
		72	+++	−	++	++S*	−	−
		96	−	−	−	++P*	−	−
14	"	24	−	−	−	+S	−	−
		48	−	"sniffles"	++	++M	−	−
		72	+++	−	++	++S	−	−
		96	−	−	−	++MP*	−	−
18	MP strain, 3rd passage	24	−	−	−	−	−	−
		48	−	−	−	−	−	−
		72	−	−	−	−	−	−
19	"	24	+	−	+	+S	−	−
		48	−	−	−	−	−	−
		72	−	−	−	−	−	−

*S = serous exudate; M = mucoid exudate; MP = mucopurulent exudate; P = purulent exudate.

Strain II (MP) was infectious for two passages, but very mild or no reactions were induced with the third and later passages when it was inoculated into human volunteers (Table 1).

Strain III (Spill) was able to induce symptoms of the common cold after 9, 11, and 12 series of consecutive passages in the developing chick embryo (Table 2). It should be noted that the later passages of this agent induced much milder symptoms in the inoculated individuals than did the earlier. This might be due, of course, either to a difference in susceptibility of the individuals who were inoculated or to an attenuation of the agent by passage through a series of chick embryos. The clinical diagnosis of cold infection in the human volunteers was based on the development of a congested and swollen nasal mucous membrane, serous exudate, and sneezing. They manifested a feeling of stuffiness in the paranasal sinus areas and of mild malaise. There was no fever and no impairment

TABLE 2

INTRANASAL INOCULATION OF HUMAN VOLUNTEERS WITH CHICK EMBRYO PROPAGATED COLD AGENT

No.	Inoculum	Hours	Symptoms: "Stuffy"	Sneezing	Swollen mucosa	Exudate	Headache	Cough	Exercise tolerance
24	Spill strain, 9th passage	24	−	−	−	−	−	−	
		48	+++	−	+++	+S*	−	+	
		72	+++	−	+++	+S	−	+	
25	"	24	+++	Sniffling	+	−	−	−	
		48	+++	−	+++red	+S	−	−	
		72	+	−	red	−	−	+	
26	"	24	−+++	−	+++	−	−	−	
		48	+++	+++	+++	++S	−	−	
		72	++	−	+++	+++MP	−	+	
27	"	24	+++	+++	+++red	++M	−	−	
		48	+++	+++	+++	++MP	−	−	
		72	+++	−	++		−	−	
28	"	24	−	−	−	−	−	−	
		48	++	−	Injected	−	+−	−	Good
		72	−	−	−	−	−	−	Good
29	"	24	+	−	−	−	−	−	
		48	++	−	+++Injected	+M	−	−	Poor
		72	−	+	−	−	−	−	Good
30	Spill strain, 11th passage	24	−	−	+	−	−	−	
		48	−	−	−	−	−	−	
		72	−	−	−	−	−	−	
31	"	24	++	Sniffling	+Injected	−	−	−	
		48	−	−	−	−	−	−	
		72	−	−	−	−	−	−	
32	Spill strain, 12th passage	24	++	−	−	−	−	−	
		48	−	−	−	−	−	−	
		72	−	−	−	−	−	−	
35	"	24	−	−	−	+M	−	−	
		48	+++	−	++	Clearing throat	−	−	
		72	−	−	−	−	−	−	
39	Haltmar strain 2nd passage	24	−	−	−	−	−	−	Good
		48	−	−	−	−	−	−	
		72	−	−	−	−	−	−	
40	"	24	+++	−	++Injected	++S	+	+	Fair
		48	−	−	−	−	−	−	
		72	−		−	−	−	−	

[1] We wish to express our appreciation to the staff and inmates of the Sugarland State Farm for their aid and cooperation in this study.

of appetite. The diagnosis was further based on negative hematological changes and negative Hirst tests for influenza A and B.

Six human volunteers were inoculated intranasally with chorioallantoic fluid (Spill strain) of the 9th passage in chick

TABLE 3

CHALLENGE INOCULATION OF IMMUNIZED HUMAN VOLUNTEERS (1cc/week/2 weeks)

No.	Intranasal challenge nasal washings (pool of Connoly, Fly & Thompson)	Hours	Symptoms						
			"Stuffy"	Sneezing	Swollen mucosa	Exudate	Headache	Cough	Exercise tolerance
24	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
28	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
29	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
30	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
31	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
41	Uninoculated control, 1.0 cc.	24	+++	Sniffling	++	++S	"Head tight"	+	Poor
		48	++	+	–	–	–	–	
		72	–	–	–	–	–	–	
32	Pool of Quattlebaum & Jones, 1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
33	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
34	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
35	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
36	1.0 cc.	24	–	–	–	–	–	–	
		48	–	–	–	–	–	–	
		72	–	–	–	–	–	–	
38*	Uninoculated control, 1.0 cc.	24	++	–	++	+++S	–	+	Fair
		48	++++	–	+++red	+++S	–	–	Poor
		72	+	–	++red	–	–	–	Poor

* This case developed an otitis media and ruptured ear drum 3 days later.

embryos. Five developed symptoms of cold, ranging from severe reactions to transitory, aborted manifestations. Five of these men and five new volunteers were inoculated subcutaneously with two doses of infectious chorioallantoic fluid (Spill strain), 1 cc. at weekly intervals. One week following the second inoculation, they were challenged intranasally with Seitz filtrates of nasal washings from naturally acquired cases of common cold infection. All of the individuals who had been immunized with the chick embryo propagated Spill strain cold agent failed to develop symptoms of cold; and with the same challenge inoculum, the two controls who had not been exposed to the Spill strain cold agent both developed marked symptoms (Table 3).

Six control volunteers were inoculated intranasally with normal chorioallantoic fluid from 11-day-old incubating chick embryos, and none developed any of the symptoms of cold infection. The same was true of five control volunteers inoculated intranasally with 2 per cent rabbit serum in physiological saline.

Development of Castes in Higher Termites

S. F. LIGHT and FRANCES M. WEESNER

Department of Zoology,
University of California, Berkeley

Studies on the determination of castes (*5*, *6*) have been confined to the lower families of termites. The higher termites, the Termitidae, seem to offer more favorable opportunities to study the mechanisms which determine the development of the structurally very different castes. They possess a readily distinguishable worker caste, and the colony consists largely of fully differentiated terminal individuals, workers, and soldiers.

The nymphs of the lower families have been shown to be indifferent up to relatively late instars (*5*). Our findings reported here agree with those of Bathellier (*1*) in placing determination of castes in the Termitidae very early in nymphal development.

The lower termites are amenable to laboratory culture, but the literature shows no records of attempts to culture the higher termites except the method for mass culturing of the sterile castes of *Nasutitermes* (=*Entermes*) *exitiosus* Hill, worked out by Holdaway, Day, etc. in Canberra, Australia. A brief report of certain features of the method was presented in 1936 (*4*) by Holdaway and again in 1938 by Day (*2*).

Although the Termitidae are largely tropical, three genera are common in the southwestern United States: *Amitermes*, of which the most common species is *A. wheeleri* (Desneux); *Gnathamitermes*, with the common and widespread species *G. perplexus* Banks; and *Tenuirostritermes*, the most common species of which is *T. tenuirostris* (Desneux). *Tenuirostritermes* is a nasute termite in which the soldiers have only vestiges of mandibles and a long, pointed projection of the head on which opens the cephalic gland.

The writers devoted the spring and summer of 1946 to studies of the last two species in southeastern Arizona, where both are abundant. Using a method reported elsewhere (*7*), we have found that the incipient colonies can readily be cultivated beyond the point necessary to obtain experimental results. Groups from older colonies present greater difficulties, *Gnathamitermes* being less difficult than *Tenuirostritermes*. More detailed reports of field observations and experimental results will be presented elsewhere.

In these Termitidae, living under essentially temperate conditions, reproduction and caste development were found to be restricted to the warmer months.

In March the only immature individuals in the colonies of *Tenuirostritermes* were the large brachypterous nymphs (with rudimentary wings), destined to become the alates which swarmed in July with the summer rains. The colony consisted almost entirely, therefore, of terminal sterile individuals, the workers, and the nasutes.

The cycle in *Gnathamitermes* differs strikingly from that of *Tenuirostritermes* in several features. In addition to the workers, soldiers, and brachypterous nymphs, there were present in colonies of *Gnathamitermes* many apterous individuals like the workers but slightly smaller and with unpigmented heads. These proved to be the late nymphal instar of the sterile castes.

Beginning in early April some of these nymphs were found to be molting into the worker stage and from May 15 callow soldiers were found in some colonies. Therefore, the older apterous nymphs found in *Gnathamitermes* colonies may be of two types, one destined to become workers, the other to become soldier nymphs; or they may be indifferent, capable of giving rise either to workers or to soldiers.

Eggs were first found in *Tenuirostritermes* colonies on April 3. On April 24 eggs were first recorded in *Gnathamitermes* and probably first appeared there a week or two earlier. Eggs were abundant thereafter in the colonies of both species until our departure in late August, and presumably for some time after that.

The existence of the restricted period of reproduction in our species of Termitidae makes it possible to determine with certainty the lines leading to the several castes, since their appearance from the first eggs may be followed chronologically.

Thus, the junior author was able to trace, in some detail, the lines of development of the different castes of *Tenuirostritermes*. Conclusions made from field observations were checked by segregating the different stages and observing in the laboratory the stage derived from each by molting. The outstanding feature of her findings was that the three major lines leading, respectively, to the nasute, the worker, and the alate types are differentiated very early in development and can be distinguished readily from a very early molt—certainly the first molt for the nasutes and probably also for the other two castes. It seems safe to say that the nasute line is determined sometime during the first stadium. Both Emerson (*3*) and Bathellier (*1*) show the nasute-like "nasute nymph" molting from a pigmented, worker-like stage, whereas we found that the nasute of *Tenuirostritermes* molts from an entirely unpigmented nymph and confirmed this origin in the incipient colony. The reduction by one in antennal segments recorded by Emerson and Bathellier was found to occur also in *Tenuirostritermes*.

Nymphs initiating the worker line arise from large-bodied nymphs probably of the first instar. These do not molt until several days after the appearance of the nymphs of the nasute line.

The time required for the development of each of the various stages of the sterile castes in the primary colonies of *Tenuirostritermes* in the laboratory agreed closely with that in the first brood of the year in older colonies in the field.

The young of the alate line, made obvious by the possession of wing buds, arise from large-bodied young nymphs indistinguishable from those which give rise to the worker line. The first wing-budded nymphs were observed in August, after the alates had flown, whereas the nasute and worker lines were being renewed from the time of the first appearance of young, about May 1.

More than 400 pairs of dealated reproductives of each species were set up and allowed to develop as primary incipient colonies. Many of these seemingly developed normally. The incipient colonies of the two species were found to differ in the development of castes in the same ways as did the older colonies in the field. Flourishing incipient primary colonies of *Tenuirostritermes* consisted of from 15 to 20 nasutes and 45 to 55 workers. In *Gnathamitermes* colonies, the numbers were smaller (20–30), and no soldiers or workers appeared, all developing to the last apterous nymphal stage and remaining in that stage for months beyond the time when all members of *Tenuirostritermes* primary incipient colonies had become either workers or nasutes.

The conditions reported here make it obvious that primary colonies of *Gnathamitermes* are not favorable subjects for experiments on determination of castes, since the nymphs do not complete their development until the following year. The same is true for *Amitermes wheeleri*, whose reproductive cycle parallels that of *G. perplexus*.

The primary colony of *Tenuirostritermes* offers certain important advantages for experimental studies. The primary pair contain all the food necessary to allow the large primary group to develop to the definitive caste, worker or nasute. This development takes place rapidly, the first worker appearing about 60 days after setting up the dealated pair and the first nasute about 3 days later. It should be possible, therefore, to test the inhibiting effect of the presence of introduced workers or nasutes upon the caste development within such primary colonies.

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Lethal Effect of X-Rays on Marine Microplankton Organisms[1]

Kelshaw Bonham, Allyn H. Seymour, Lauren R. Donaldson, and Arthur D. Welander

Applied Fisheries Laboratory, University of Washington, Seattle

Published literature concerning the effect of X-rays upon free-living microorganisms has dealt chiefly with fresh-water life. Ralston (*1*), however, found dosages of about 18,000 r lethal to *Dunaliella salina*. The present experiments deal with four genera of Protista obtained through the courtesy of Vance Tartar, of the State of Washington Department of Fisheries. These may be designated as (1) *Chlorella* sp. (Loosanoff's culture), a 2-μ, spherical green alga; (2) *Nitzschia closterium* (Loosanoff's culture), a diatom; (3) an unidentified green car-

[1] This paper is based on work performed under contract No. W-28-094-eng-33 with the Manhattan District-Atomic Energy Commission.

teriid mastigophoran 14 μ long with four flagella, from Burley Lagoon, near Gig Harbor, Washington; and (4) an unidentified brown mastigophoran 8–10 μ long with two long flagella, from Rosedale, Washington.

On February 28, 1946, seven 5-cc. samples, taken in a random manner from each of the cultures, were removed to 1-dram shell vials for experimentation. The cultures filled the vials to a depth of about 38 mm. Two of each series, the first and the last removed, were used as controls. The other 5 vials were subjected at the rate of 132 r/minute to doses in roentgens of 1,000, 10,000, 25,000, 50,000, and 100,000. X-rays were produced at 200 kv. and 20 Ma. with filters of 1 mm. of aluminum and 0.5 mm. of copper. The target distance was 13 inches as measured to the middle of the depth of the culture in the vials. The vials were arranged in a cluster of 4-inch maximum diameter. On March 1 a second series of cultures was also exposed to the same amount of radiation, but in order to shorten the time of exposure the rate was increased to 530 r/minute by halving the target distance to 6.5 inches.

The number of swimming brown flagellates was determined by the use of blood-counting chambers immediately after the removal of the last vial from the X-ray machine. The dose that was immobilizing to 50 per cent of these organisms within 24 hours appeared to be about 50,000 r, while 100,000 r was almost 100 per cent effective. Viability was not readily ascertained in the other species. After irradiation the cultures were transferred to larger vials of 6-dram capacity, and 12 days later were subdivided, about two-thirds of each being removed to an 8-ounce jar. Several days later, when the culture had evaporated to about half of its original volume, the loss was compensated by adding sea water containing 0.1 per cent of milk. Population densities in the cultures were determined at irregular intervals by counting the number of organisms per unit volume of culture, using ruled blood-counting chambers.

TABLE 1

SUMMARY OF FIRST MONTH'S COUNTS EXPRESSED AS PERCENTAGE OF HIGHEST COUNT

Dose in r	*Chlorella*	*Nitzschia closterium*	Carteriid	Brown mastigophoran
0	100	100	80	29
0	83	72	100	100
1,000	94	67	67	66
10,000	75	34	61	37
25,000	58	20	46	0
50,000	43	12	14	0
100,000	23	8	81	0

The results shown are based on counts made only during the first month of the experiment. For the first 45 days there was general agreement in the effect upon all four organisms. Later tendencies of the counts extending to 5 months were erratic or obscure, possibly because of contamination of cultures. The concentrations of each organism at the various dosage levels are compared in Table 1. To arrive at these figures for a species, the counts in each irradiation group were averaged and then expressed as percentage of the highest average count for that species. The carteriid counts at the 100,000-r level were high and suggest faulty technique, but other counts were in general inversely related to amount of exposure above 1,000 r. The counts of cultures exposed to 50,000 r and 100,000 r were low. Counts of cultures exposed to 1,000 r were not unlike the controls in that in 9 cases these were greater than, and in 10 cases less than, the controls. On this same basis of 19 counts for each culture the times that the 10,000 r treatment exceeded the control were twice; for 25,000-r, once; for 50,000 r, none; and for 100,000 r, once.

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The Contagious Nature of a Lymphoid Tumor in Chickens

NELSON F. WATERS

U. S. Regional Poultry Research Laboratory, East Lansing, Michigan

Nearly all treatises on tumors stress the point that little foundation exists in the theory that a tumor, or any agent responsible for a tumor, is communicable in nature. Arrayed against this theory of noninfection is the ever-increasing amount of data suggesting a virus etiology for certain tumors. The specificity of certain of the viruses and the indisputable evidence of infection, direct or indirect, that is associated with virus diseases, lead one to continue the search for a virus or virus-like agent which causes tumors and which is transmissible by contact with infected animals. Again, the knowledge which has been gained in an overall study of other diseases leads one to believe that, in the genesis of specific types of tumors, some orderly pathogenic stimulus is necessary to incite, directly or indirectly, neoplastic growth. Likewise, the myriad of seemingly spontaneous tumors occurring in the animal kingdom exists because certain necessary requirements of host-pathogen-environment relationship have been fulfilled.

In strains of mice susceptible to pulmonary tumors, it is not unusual to find that approximately 90 per cent of the animals at 18 months of age show "spontaneous" tumors (*3*). A description of these indicates such a similar host reaction that it is difficult to believe they were not activated by a common agent and that this agent, through some as yet unexplained manner, can find a portal of entry from one animal to another. Bittner (*2*), Andervont (*1*), and others have shown that mammary adenocarcinomas in mice were due primarily to the relationship between the genetic influence, the hormonal influence, and the milk influence in certain individuals. Discovery of an agent responsible for mammary tumors in mice was the result of carefully designed objective experimentation which included the development of host animals of similar genetic potentialities.

Among the important factors working against any experiment attempting to prove the communicable nature of agents responsible for tumors are the following: (1) If a tumor agent were transmitted by contact from one animal to another, the length of time which elapses before a tumor is grossly visible is generally too great to ascertain that such contact had any relationship to the tumor eventually formed. (2) It is almost impossible to be sure that the experimental animals are actually free of any tumor agent prior to the time of exposure. (3) There are not available enough experimental inbred animals which

are uniformly free of, but susceptible to, the tumor agent studied. Until these and many other variables are controlled, the chances of proving contact transmission are slight indeed.

Research at the U. S. Regional Poultry Research Laboratory, carried on in active collaboration with a number of experiment stations within the region, is involved in an attempt to control lymphomatosis in the domestic fowl. Lymphomatosis is a naturally-occurring malignant disease of chickens characterized by the formation of lymphoid tumors which may be found in nearly all tissues of the bird's body. A review of the literature indicates that the lymphoid tumors associated with this disease are generally considered to be neoplastic entities.

Initial studies at this Laboratory started in 1939, at which time only hatching eggs were introduced onto the premises. Chickens obtained from these eggs were housed in completely new buildings. Exacting quarantine and sanitary measures provided for in all experimental work have been successful over a period of 8 years in preventing all common infectious diseases except lymphomatosis and all parasites of chickens except coccidiosis.

The first naturally occurring case of lymphomatosis at the Laboratory was observed about 40 days after the first chickens were hatched. During the first 600 days of life about 22 per cent of the chickens in this first population developed gross lymphoid lesions typical of the disease. For the past 7 years 20–40 per cent of the birds in each population have shown gross evidence of naturally occurring lymphomatosis. The presence of lymphomatosis in the first population, and the more or less complete absence of all other infectious diseases and parasites, provided very strong evidence of egg transmission, but it was not positive enough to remove all doubt. Accordingly, experiments were designed to provide more proof of such transmission.

The procedures followed consisted in selecting the eggs from certain hens and incubating, hatching, and rearing the resultant chickens by groups of families (a family consists of progeny of one sire and dam) in complete confinement and under extremely rigid quarantine (*8*). Altogether, 22 families of chickens were tested, and at the end of a 300-day period 9 of these (64 birds) had one or more individuals (a total of 16 per cent) showing gross lymphoid tumors, while 13 families (92 birds) showed no evidence of tumors. Sib controls of each of these 22 families were incubated, hatched, and reared with the main population of 1,074 chickens, of which approximately 34 per cent developed gross lymphoid tumors at 600 days of age. The 47 exposed sib controls of the 9 families showed 32 per cent with tumors; the 83 exposed sib controls of the 13 families, 21 per cent.

A second test consisted in selecting 4 females and 1 male, all sibs, from 1 of the 22 families showing no tumors. A mating (maintained in strict pen isolation) of these sibs produced 4 families consisting of 91 progeny which were incubated, hatched, and reared by families each in individual pen isolation. Control sibs and half sibs (121 in all) of these 4 families were incubated and reared with the mixed infected population consisting of 1,059 chickens. At the end of 300 days none of the isolated chickens in the 4 families showed gross lymphoid tumors, and 1 family of 17 chickens is still alive at 1,800 days of age. In contrast, 32 per cent of the exposed sibs and 20 per cent of the main population developed gross lymphoid tumors.

In a third test 4 female sibs were selected from among the progeny of the second-test chickens. Similar to the second test, these 4 females were mated to a brother, the resultant progeny again being incubated and reared by families, each in complete pen isolation. In this test at about 200 days of age, certain birds from 1 of the 4 families were diagnosed, upon gross necropsy, as showing questionable lymphomatosis, and the entire family was summarily destroyed. Of the remaining 3 families, consisting of 72 chickens, all were alive and apparently free of tumors at 350 days, at which time 23 birds were killed and no tumors found. The 49 remaining in this sample were killed at about 500 days of age, and only 1 bird showed gross lesions which could have been typical of lymphomatosis. About 44 per cent of the control sibs (165 birds) of these third-test families had gross lymphoid tumors at 500 days of age when exposed to a mixed infected population (1,014 birds), 35 per cent of which had developed gross tumors during the same period.

From these experiments several important conclusions relative to the communicability of this disease are justified: (1) Certain families of chickens highly susceptible to lymphomatosis can be reared either completely or relatively free of lymphoid tumors as well as all other recognized diseases and parasites (*8*); (2) the families of chickens showing no tumors were highly susceptible to lymphomatosis (*5*); (3) lymphomatosis is apparently a communicable disease, and the agent presumably responsible for it can be transmitted readily from bird to bird (*6*, *8*); and (4) the egg is incriminated as a carrier of the agent responsible for the disease (*6*, *8*).

The suggestion that a chicken tumor having the properties commonly associated with neoplasia is caused by an agent which is contagious is unique in the history of neoplasia as found in other animals. This raises the question as to whether the lymphoid tumors which characterize lymphomatosis in chickens do represent malignant neoplastic growth as described in other animals. Shimkin (*4*) cautions: "Agreement that the condition represents a malignant neoplasm is desirable, although it is realized that the criteria for malignancy are far from satisfactory and far from being sharply defined."

The experimental techniques used at this Laboratory to obtain inbred chickens relatively free of lymphoid tumors provide thought-provoking information on natural transmission, both by way of the egg and by bird-to-bird contact, and also information on the genetic resistance of the host (*5*, *7*). How valuable free, susceptible chickens could be to oncological studies in the fowl is self-evident. If inbred chickens highly susceptible to specific lymphoid tumors, when reared by families in strict pen isolation, develop relatively few or no tumors, it can only be assumed that the actuating causes which promote tumor growth were not prevalent either in the chickens or in the environment surrounding them.

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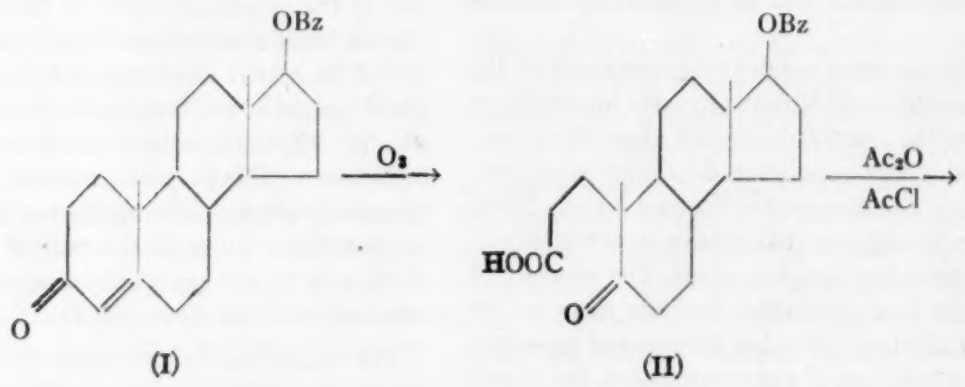
IN THE LABORATORY

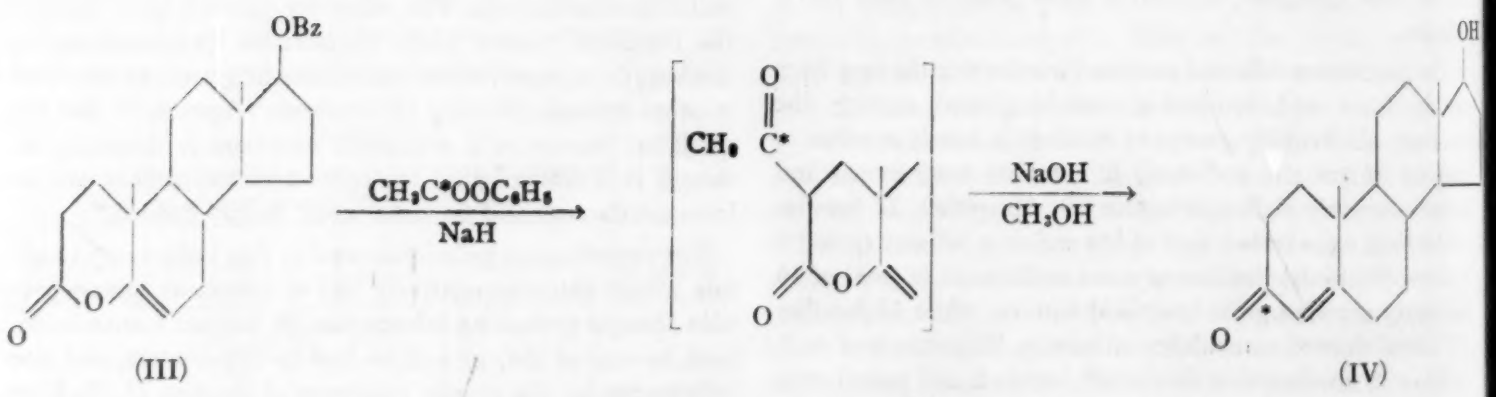
Radioactive Testosterone[1]

RICHARD B. TURNER

Department of Chemistry, Harvard University

A method for the preparation of steroids labeled with isotopic carbon in ring A has recently been reported from this laboratory (*3*). This work has now been extended to the preparation of 3-radiotestosterone (IV), which is the first example of a synthetic radioactive sex hormone.





The procedure employed, outlined in the accompanying diagram, was essentially that reported previously for cholestenone (*3*). Ozonization (cf. *1*) of testosterone benzoate (I) afforded the keto acid II (m.p., 147–148°; $[\alpha]_D = +79°$) in a yield of about 65 per cent. *Anal.* Calc'd for $C_{25}H_{32}O_5$: C, 72.79; H, 7.82. Found: C, 72.83; H, 7.84. This substance was treated with acetic anhydride and acetyl chloride, and an enol-lactone (III) was obtained (80 per cent yield) which melted at 202–202.5°; $[\alpha]_D = -19°$. *Anal.* Calc'd for $C_{25}H_{30}O_4$: C, 76.11; H, 7.66. Found: C, 75.98; H, 7.74.

Condensation of III with phenyl acetate containing C^{14} in the carboxyl group proceeded smoothly in the presence of sodium hydride, yielding an oil which was treated directly with a methanolic solution of sodium hydroxide. The product, isolated by chromatographic separation on alumina, melted at 153–154° and did not depress the melting point of an authentic sample of testosterone. The identity of the substance was further established by measurements of specific rotation (+110°) and ultraviolet absorption ($\lambda_{max.} = 241$ mμ, $\log_{10} \epsilon = 4.21$). Testosterone acetate (m.p., 139–140°) was prepared as a derivative.

The above condensation reaction gave testosterone with an activity of 1.45×10^4 counts/min./mmole[2] in a yield of 48 per cent based on the enol-lactone (III). This substance and compounds of a similar nature may be expected to be of importance in connection with investigations of the intermediary metabolism of the steroid hormones and the relation of these hormones to cancer.

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[1] This work was supported by funds provided by the American Cancer Society on the recommendation of the Committee on Growth, National Research Council. The testosterone used in the investigation was made available through the generosity of Erwin Schwenk, Schering Corporation, and C. R. Scholz, Ciba Pharmaceutical Products, Inc.

[2] The author is indebted to Warren W. Miller, Radioactivity Center, Massachusetts Institute of Technology, for activity measurements. For a description of the counting method see reference *2*.

Inhalation of Penicillin Dust

LOUIS KRASNO, MARY KARP, and PAUL S. RHODES
Wesley Memorial Hospital, Chicago, Illinois

In view of the favorable reports in recent literature on the inhalation of therapeutic agents, especially penicillin, and because of the many possibilities of the method, we have attempted to produce even greater and more protracted topical effectiveness by a method which involves the inhalation of the fine dust aerosol particles. This method has simplified the mechanics of aerosol therapy, thus expanding its scope more readily for office and home treatment.

Apparatus used for inhalation therapy in the past has been based on the delivery of the aerosol vapor under positive pressure of oxygen or air by means of a hand bulb or oxygen tank and gauge. The principle utilized in the new method is based on the negative pressure created by normal breathing during the inspiratory phase. Inhaling penicillin dust in this manner is more physiological and permits a more even and perhaps a wider distribution of medicament throughout the respiratory tract. The patient is not required to manipulate an exhaling valve during the expiration phase, and the cumbersome equipment of oxygen tank and gauge is unnecessary. The inhalation of the dust particles yields a greater effect than the vapor, because there is greater concentration of penicillin per unit area and because the penicillin must go into solution while in contact with mucous membrane before it can be absorbed.

The penicillin dust used in this study was crystalline sodium penicillin[1] processed to # 50–100-mesh particles. This was found to be less hydroscopic than the sodium penicillin salt and could be stored at room temperature, without losing its potency, more readily than the penicillin liquid which was used as the nebulin for the vapor method.

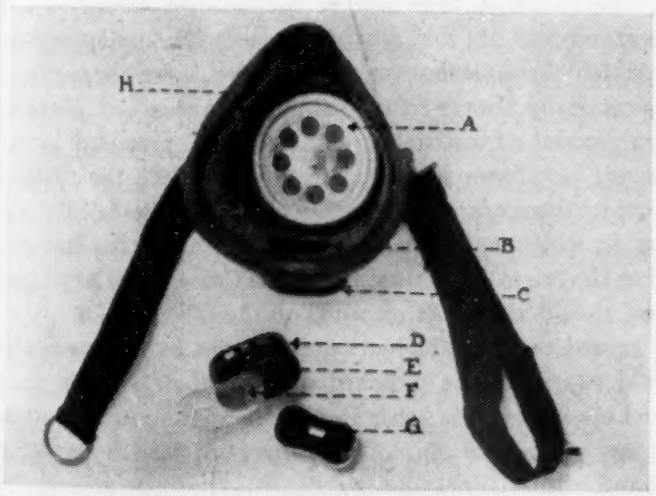

FIG. 1

The apparatus[2] represents numerous modifications of the original device, since penicillin dust possesses certain qualities which do not lend themselves to ordinary techniques. Penicillin is highly electrostatic, easily adhering to surface areas. In addition, it is hydroscopic and goes into solution readily. The technique finally developed allows the penicillin to be easily released as a dust and at the same time precludes the moisture of exhalation from dissolving the dust.

[1] The penicillin for this study was processed by, and furnished through the courtesy of, the Abbott Laboratories, North Chicago, Illinois.

[2] The present mask was kindly fabricated by the Chicago Eye Shield Company, according to specifications of one of the authors (Krasno).

The device (Fig. 1) consists of an oronasal facepiece which is held on by an elastic headband. The front of the mask contains a large exhalation valve (A) in direct line with the stream of exhaled air. The portion of the mask about the chin contains a rubber sleeve (C) fitted with an inhalation valve (B), which directs the inhaled air in a plane at right angles to the stream of exhaled air. This valve contains a special detachable metal chamber (D) containing the penicillin dust. The penicillin chamber is fixed with a fine wire-mesh platform (E), which is covered with a finely perforated cellophane paper (F). It has been found that penicillin dust is essentially nonadherent to cellophane. The dust is placed on top of this platform. During the inhalation, air drawn through the perforations of the penicillin chamber releases the dust, which passes through the inhalation valve and into the respiratory passages. The dust is protected from the moisture of exhalation by the one-way inhalation valve and by the fact that the exhaled air is conducted through a plane at right angles to the inhaled air. A small hole (H) about 6 mm. in diameter, on top of the mask just above the end of the nose, allows a substantial amount of air to be drawn in easily and avoids any feeling of "resistance" on inspiration. This supplementary inlet for air is also necessary to keep the amount of penicillin dust inhaled in optimum quantities.

This preliminary work, begun in October 1946 and extended through February 1947, comprises studies of 68 subjects (38 males and 30 females). Four experimental subjects (including the authors) inhaled the penicillin dust in varying proportions in order to detect any untoward effects or irritating properties. Fifty-five patients had upper respiratory infections: 46, symptoms of the "common cold"; 4, chronic nasopharyngitis; 3, chronic sinusitis; and 2, laryngotracheal bronchitis. Nine patients had lower respiratory tract infections: 6, bronchiectasia; 2, chronic bronchitis; and 1, bronchial asthma.

All patients received 100,000 units of penicillin dust by inhalation for 20 minutes three times a day. Throat and nose cultures prior to first treatment and daily cultures thereafter were taken on all hospitalized patients. Sputum cultures were taken in cases of bronchiectasia. Single blood-level determinations were made on various patients in this group. The duration of therapy was determined clinically and bacteriologically for each case.

The inhalation of dry penicillin quickly causes the reduction or disappearance of gram-positive bacteria and, in the massive doses used, seems to effect some of the gram-negative bacteria. This is accomplished readily, often in one treatment. Penicillinase has been added to the laboratory specimens without changing the bacteriologic results.

The clinical results were based on the patient's subjective improvement, the opinion of attending doctor, and the impressions of the authors. A study of the data of the 6 bronchiectatic patients revealed less cough and secretion on dismissal. The patient with bronchial asthma showed marked improvement, the infectious condition almost completely disappearing, although the allergy persisted to a decreased degree. The 2 patients with chronic bronchitis showed a moderate improvement. The symptoms of over 70 per cent of those patients with acute conditions of the upper respiratory tract cleared up markedly, in many instances after only one or two treatments. Seventy-five per cent of the patients with upper respiratory tract infections had only one treatment. Six had two inhala-

tions, and three had four treatments. The remainder had 4–18 inhalations, the highest number being reserved for the chronic bronchitis and chronic sinusitis. The range of duration of treatments for the patients with diseases of the lower respiratory tract was 10–18 treatments, administered in from 4 to 6 days.

It is singular that up to the present time no sensitivity reaction to the penicillin has been noted. With the vapor aerosol method, transient fever, dyspnea, or dermatitis has occurred in 4–20 per cent of the cases, depending upon the concentration of the drug used.

Penicillin assays of urine and blood were determined by the Flemming modification of the Wright slide cell technique. Values ranged from .03 to 1.92 units/cc. Blood-level curves would indicate a slow absorption, the maximum level being obtained 3-3½ hours after the inhalation.

This new method of administering penicillin appears to offer more effectiveness than other aerosol methods in ridding the upper respiratory tract of gram-positive bacteria. Appreciable blood levels would indicate the protracted effective absorption offered by this method, and the possible use of inhalation treatment for systemic conditions other than those of the respiratory tract. Although the method would appear to have a definite value in initiating the process of ridding the respiratory tract of pathogens, it may be necessary to supplement the process with other medicaments. In other instances its value may be adjunctive rather than primary.

Formaldehyde in Plant Collecting

F. R. Fosberg

1631 Liholiho Street, Honolulu, T. H.

Recently Schultes has published an intelligent and much-needed discussion of the use of formaldehyde in the preparation of herbarium specimens, recommending its use to prevent disarticulation, molding, and decay when immediate drying is impractical (*Rev. Fac. Agron.* (Medellín, Colombia), 1946, 6, 46-52; *Rhodora*, 1947, 49, 54-60). His development of this method resulted from a conversation with Paul H. Allen, who has for some years employed alcohol and formaldehyde in the preparation of his excellent specimens of Panamanian plants.

Since I was present at the conversation referred to and have independently developed a somewhat different formaldehyde technique, it seems worth while to supplement Schultes' papers with an account of some of the possible variations and certain minor drawbacks, as well as to publish the method where it will reach a wider audience.

Allen's original method was to spray the freshly pressed specimens, using a common Flit gun, with a mixture of 70 per cent alcohol and enough formaldehyde to give it a strong odor. I tried this, but found that the specimens still molded and deteriorated to a certain extent, especially when final drying was delayed for a considerable time. Spraying with a Flit gun had the additional disadvantage of practically pickling the collector as well as his plants.

Much experimenting finally resulted in a mixture that seems to work under most conditions and has one big advantage over the straight formaldehyde-water solution advocated by Schultes. This mixture consists of approximately 1 part each of concentrated (40 per cent) formaldehyde, 95 per cent alcohol and water. If 70 per cent alcohol only is available, as in many small towns, I commonly use 1 part formaldehyde and 2 parts alcohol. The alcohol gives the mixture much better wetting properties than those of a straight water solution. Not only does it readily wet even waxy leaves and those with a prominent coating of hair or scales, but it also penetrates much more quickly and thoroughly.

After I had suffered an atmosphere of formaldehyde spray from a Flit gun for several trips, Norman C. Fassett suggested applying it with a small paint brush. This method was much more effective and lacked the unpleasant features. I had already discarded the dipping method as rather ineffective and clumsy at best, and completely unsuited to plants that tended to be limp. Of the three methods, use of a soft, 2-inch paint brush is by far the most satisfactory. H. H. Bartlett (in a letter) has suggested a rather different method which consists essentially of tying a fair-sized bundle of specimens together in folded newspapers, standing them on end, and pouring a sufficient amount of the solution described above into the bundle to wet it.

It is usually suggested that after the application of formaldehyde the bundles of specimens be wrapped in oilcloth, waxed paper, pliofilm, double canvas, or other material to retard evaporation. If specimens are to be mailed, this is doubtless necessary, since otherwise they would probably not be accepted by postal authorities. However, I have found that there are no evident bad effects if they are left simply wrapped in either wrapping paper or several thicknesses of newsprint. The solution largely evaporates off after killing the tissues of the plants as well as the spores of such fungi as are likely to cause trouble. The dead tissues lose their water much more readily than do living ones, and drying goes on to a certain extent even in the bundles. Subsequent drying over heat also takes place much more rapidly for this same reason.

Those to whom the color of the finished specimens is of importance will not find any of the methods employing either formaldehyde or alcohol very satisfactory, since the specimens are practically always either discolored or bleached. However, few collections from wet tropical regions, prepared by any method, have been preserved in anything like their original colors; furthermore, a few years in the atmosphere of the large cities in which our herbaria are unfortunately located soon makes the original color unimportant. Colors, if of any importance, should always be recorded on the labels.

I regard the formaldehyde method, not as something that should supersede the ordinary method of drying the plants over heat after a few hours in a tight press, but as a supplementary method for use in places where ordinary drying is difficult and with material that is not satisfactorily prepared by the ordinary methods. Leaving the material in a wet condition for any considerable length of time cannot have a good effect and probably results in a certain amount of brittleness. However as Schultes very effectively points out, many groups of plants that make traditionally bad specimens give very gratifying results with this technique. One has only to look into the covers of almost any genus of tropical mimosoid legumes to be convinced that improved methods are in order. Certain of these which were not dried for three weeks after gathering show no tendency whatever to lose their leaves. The formaldehyde apparently completely and instantaneously stops the formation of abscission layers. It might be well to try this technique on such conifers as *Tsuga* and *Picea* and on the almost mature cones of

bies, of which I have seen few satisfactory specimens because of the dropping of leaves and cone scales on drying.

The tissue-killing effect of this solution also lightens immeasurably the burden of preparing material of such difficult plants as Crassulaceae, Portulacaceae, Aizoaceae, and other fleshy groups, even in a dry climate.

Direct Introduction of Chemical Substances into Herbaceous Plants[1]

IRMA M. FELBER

Department of Horticulture, Michigan State College, East Lansing

During studies concerned with the effect of 2,4-D upon plants the need for a means of introducing chemicals into any desired part of herbaceous plants, without either injuring the plant itself or taking up too much of the experimenter's time, became obvious. The principal methods of treatment hitherto used have major drawbacks.

The solution to be tested is placed upon the upper surface of a leaf blade either in the form of a drop or in a small disk of filter paper (*2, 3*). Also, pellets containing mixtures of a paste-like carrier and growth regulators have been used (*1*). Necessarily, these treatments are limited to those organs of the plant which, by virtue of their shape, support a drop of liquid or a disk of moist filter paper. They are not feasible for application to erect or inclined parts or to surfaces of a repellent or impermeable nature. The injection method (*4*) is not practicable with herbaceous plants in large-scale experiments because it is too time consuming; also, the inner turgor of the tissues usually prevents penetration of additional fluids from outside, and the cells are readily injured or ruptured when even a minimum of liquid is forced into them.

A good deal of knowledge is still lacking with regard to the fundamental facts about the mechanism of growth, metabolic systems, and correlations between physiological and morphological developments in plants. This ignorance may be due to inadequate means employed in tracing and analyzing various phases of reactions which take place in the intact growing plant.

Attention is called here to a technical device used by Winifred O. Roberts (*5*) in diagnosing mineral deficiencies in leaves, and also developed independently by the author for experiments with plant growth regulators. The fact that essentially the same method was conceived by researchers of different countries seems to confirm the need for, and the versatility of, the technique.

Needles formed from florist steel wire No. 30 are threaded with 2–3 inches of white mending cotton. The thread is soaked to saturation in the solution to be tested and drawn through the desired part of the plant. The needle is then severed from the thread, the latter being left inside the tissue (Fig. 1). If a continuous supply of a substance is desired, the end of a longer thread may be left immersed in the solution, thus serving as a wick. Powdered substances may be introduced similarly by making them adherent to a slightly moistened thread. If the exact amount of introduced substance is to be determined, the thread may be dried after soaking and the differences in weight of the treated and untreated threads calculated for any given length. It has been observed that a thread impregnated with a solution of 2,4-D, dried, and then introduced is equally effective.

There was no instance of a detectable effect from puncturing the tissues with the needle or introducing a thread soaked in distilled water. In order to avoid contact of the plant sap with metal, the wire may be replaced by a plastic or glass-fiber material. The time factor involved in using this method for experimental purposes is negligible. An unskilled worker can prepare 60–80 needles per hour, and the treatment of 150 plants requires about one hour.

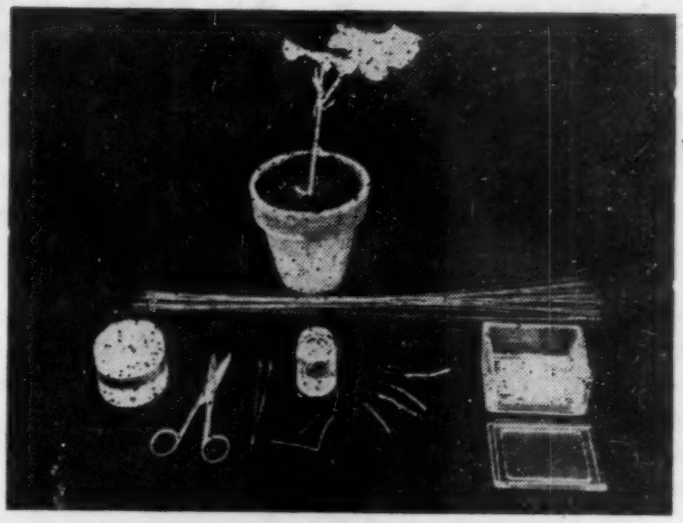

FIG. 1. Thread introduced in the first node of a bean seedling (above), and tools used in preparing threaded needles (below): wire, mending cotton, scissors, flat forceps, and glass bottle for soaking threads. Square glass jar (right) is used for storage of threaded needles.

The "thread method" already has been used successfully in experiments conducted to test (1) the responsiveness of specific morphological parts of bean plants to 2,4-D, (2) the transport of 2,4-D within the plant, (3) the effect of 2,4-D in relation to acid and alkaline systems of plants, and (4) the pH in the growing plant, by using threads impregnated with indicator solutions. In some of the experiments several thread tests involving various time intervals between the single treatments, have been used on the same plant. A detailed report of these studies will be published elsewhere.

This method seems to be useful in cases where (1) specific areas of a plant cannot be reached by applying a drop, (2) minimum amounts of a substance are to be introduced, and (3) the capacity of specific tissues is to be tested relative to absorption, accumulation, or transport of metabolic or artificially introduced substances. The method offers innumerable possibilities of experimental tests with normal and diseased plants and may facilitate new discoveries of both morphological and physiological correlations and the fundamental mechanism of growth.

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Book Reviews

Handbuch der Katalyse. Vol. 3: ***Biokatalyse.*** G. M. Schwab. (Ed.) Vienna: Springer, 1941; Ann Arbor, Mich.: J. W. Edwards, 1945. Pp. 622. $19.00.

This extremely valuable volume contains five parts, all of which are of considerable interest to the enzymologist. The opening section (46 pp.), by A. Schäffner, is concerned with the general properties of enzymes, their preparation, and the nature of enzyme action. The second and longest section (245 pp.), prepared by H. Kraut, M. Rohdewald, Ä. Weischer, and E. Kofranyi, deals with the hydrolysing enzymes, which are classified and are individually considered in terms of their occurrence, estimation, preparation, and properties.

The third section, which is the only one printed in English, is a complete (214 pp.) and well-documented treatise on biological oxidation-reduction catalysts by K. A. C. Elliott. Although the book is now several years old, this section is an invaluable aid to the investigator who wishes to obtain a detailed account of the oxidative enzymes as a basis for the study of current reports. Students and teachers will find a wealth of material that is not available in any other more recent source. The section includes 8 chapters, one of which is an excellent chapter on "Carriers."

The fourth section, by A. Schäffner and H. J. Jakowatz, is entitled "Viruses From the Standpoint of Catalysis and Autocatalysis" (39 pp.). It is concerned mainly with the mechanism by which the normal cell proteins are rebuilt into virus protein.

The fifth section, by G. M. Schwab and F. Rost, is concerned with enzyme models (37 pp.) and provides a summary of many catalyses which simulate enzymatic catalysis.

The volume includes an extensive author and subject index.

VAN R. POTTER

McArdle Memorial Laboratory, University of Wisconsin

Inorganic syntheses. (Vol. II.) W. Conard Fernelius. (Ed.-in-Chief.) New York-London: McGraw-Hill, 1946. Pp. xii+293. (Illustrated.) $4.00.

This second volume of a series giving tested procedures for the preparation of inorganic substances is characterized by the same high standards as the well known Volume I, and it should be on the bookshelf of anyone who has occasion to prepare or purify inorganic substances. The great virtue of the book, like its predecessor, is that every procedure has been critically evaluated and tested independently in a laboratory other than that in which it originated. The names of the original authors as well as those who checked the method are given under each procedure. In this connection the following quotation from the Preface is pertinent: "Inasmuch as the syntheses in Inorganic Syntheses originate from individual contributors, the editors have been perturbed to find specific references to Volume I that do not mention the individual contributor. The editors prefer that Inorganic Syntheses be considered as a periodical when references are given and that credit be given to the contributor."

The syntheses are arranged in 8 chapters, numbered according to the groups in the Mendeleev periodic table. Each of the 81 methods given starts with a brief introduction, which is followed by a detailed procedure and finally by a short discussion of the properties of the substance concerned. Each synthesis concludes with key references to the original literature. In many instances two or more alternate procedures are supplied. Very well-executed drawings of special apparatus accompany many of the procedures.

Since the inclusion of any synthesis in a given chapter on the basis of the periodic table is somewhat arbitrary, a list of syntheses which are presented in other chapters but which might have been included in the chapter in question are given at the beginning of each chapter. Cumulative subject and formula indexes to both volumes render easy the location of any particular syntheses.

The nomenclature adopted in this volume conforms, in general, with the rules recommended in 1940 by the Committee for the Reform of Inorganic Chemical Nomenclature of the International Union of Chemistry, and with *Chemical Abstracts* usage. These rules are explained by Janet D. Scott in a 10-page appendix.

JAMES J. LINGANE

Department of Chemistry, Harvard University

General biology. (2nd ed.) Perry D. Strausbaugh and Bernal R. Weimer. New York: John Wiley; London: Chapman & Hall, 1947. Pp. viii + 718. (Illustrated.) $4.75.

After a concise treatment of protoplasm and the cell, the second edition of Strausbaugh and Weimer's *General biology* proceeds rapidly to a consideration of carbohydrate synthesis in plants. The succeeding chapters deal in logical sequence with digestion and absorption, food transportation, respiration, and excretion in both plants and animals. In a text so complete and well illustrated as this, no objection should be offered to the parallel consideration of transpiration in plants and excretion in animals as long as adequate contrasts are drawn. The new chapter on hormones and auxins is thoroughly up to date. Growth and reproduction are considered in the same chapter, and meiosis is postponed wisely until the chapter on heredity.

The usual survey of the animal and plant kingdoms is well balanced within the confines of 200 pages. Ecology and evolution are given a broad presentation in the next two chapters.

The style of the book attains conciseness without the dull, laconic quality sometimes observed in texts covering so broad a field. Physiological concepts are stressed at a level that is neither superficial nor confusing in technicalities.

Index and glossary are combined, and bibliographies are eliminated except for occasional footnotes. Long book lists serve no practical purpose, since most instructors prefer to select their own supplementary readings.

The typography and well-labeled illustrations are excellent and, for a full-year course, the text is highly recommended.

GEORGE L. CHURCH

Brown University, Providence, Rhode Island